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The Coefficient of Safety in Navigation

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THE COEFFICIENT OF SAFETY IN NAVIGATION.

BY PROF. W. A. ROGERS, OF CAMBRIDGE (U. S.) OBSERVATORY.

It is customary among engineers and architects, in making allowance for the strain to be borne by any part of a structure, to assign to the materials used a strength sufficient to withstand a strain much greater than that to which the structure is ever likely to be subjected. By a combination of theory and experiment it can be found, for example, what is the *breaking load* of a wooden or iron beam of given dimensions, and from the empirical law thus established we can ascertain approximately the breaking load for any beam of the same material and having the same form and dimensions; but in order to cover all possible differences which may exist among different beams, a coefficient of safety is either introduced into the formula representing the breaking load, or is applied to the result obtained from the formula. This coefficient should be large enough to cover not only the largest possible deviation between theory and experiment, but also to meet all unforeseen emergencies, such as time and age inevitably bring.

Passing now to the consideration of the term *coefficient of safety* as applied to navigation, it is our object to find the limits within which, under ordinary circumstances, a vessel can be located at sea,

and then to adduce some considerations which will enable us to form an intelligent judgment in regard to the *range of error* to which observations are liable. The quantity wanted is the average number of miles of error in latitude and longitude which we may fairly charge upon a single observation at sea under ordinary circumstances. We have then to find the coefficient by which this number must be multiplied in order to secure absolute safety, as far as safety depends upon human means and exertions. If we represent the average error by E , and the safety coefficient by P , then $E \times P$ should cover not only the range of error to which observations at sea are liable, but also those extreme cases in which a combination of causes acting in the same direction may produce errors of greater magnitude than the most prudent navigator would even suspect. We start with the average error E , in a given series of observations. It is obvious that the absolute error may be as small as the least value of the series and as large as the greatest value. A constant error may also be involved in every observation of the series, even when there are no indications of its existence. The total error therefore may be:

$$E \times P + \text{a constant.}$$

In this investigation the computed value of the coefficient P is obtained by dividing the difference between the greatest and the least values of a given series representing errors of observation by the average value.

Capt. Williams of the ill-fated Atlantic is represented by a reporter of the New York Tribune to have said that he knew the position of his ship within her length, at the time of his last observation, twelve hours before she became a wreck. If he really made that statement, no other evidence is needed of his utter unfitness for the position which he occupied. After twelve hours' run his error was twelve miles. Was that so large an error as to justify the charges with which the daily press teemed, that he had "blundered in his calculations"? In the court of investigation which followed the disaster, was the question how far it might have been allowable for him to have been in error, raised? Did the daily press raise this question? Was the question ever considered in any court of inquiry? Not as far as I can ascertain. Was this aspect of the question ever investigated at all upon any intelligent basis? If it has been, I have failed to find a record of it, after the most diligent search. In no one of

the forty-three volumes of the English Nautical Magazine, a standard authority, to which I am largely indebted in this discussion, can I find the question once raised, and I think there are few articles bearing upon the subject of wrecks which I have not consulted. In the British Admiralty laws on this subject, especially in the new code adopted in 1849, after the repeal of what is known as Huskinson's law, almost every aspect of the question is considered except this. There are regulations upon almost every conceivable point, from the constitution of courts of inquiry to the furnishing of proper lime-juice for seamen. In the Wreck Register, published annually by the British Board of Trade, to which I am largely indebted for data, not only the number, but the causes of wrecks are enumerated. A small number are attributed to the incapacity of masters, but even here there seems to have been so far no inquiry as to what constitutes incapacity in this respect. If it is a fact that navigators proceed upon the supposition that they can with certainty obtain their position by astronomical observations within one mile, to say nothing of three hundred feet, the wonder is, not that so many wrecks occur, but that more do not occur. I have said I could find no mention of this aspect of the question. Perhaps I ought to except two editorials on the loss of the City of Washington. The New York Times said: "It is believed that the captain of an ocean steamer, freighted with a priceless cargo of human life, should know at all times, in spite of fog and tempest, just where he is." To which the New York World replies, that "any one who believes such stuff as this is an idiot, and that the only way for him to find out anything about the Nova Scotia coast would be by taking command of a ship freighted, not with a priceless cargo of human life, but with a crew of cheap and congenial idiots, and required to know in spite of fog and tempest just where he was." One might almost say that the 19th century is behind the 18th in a proper appreciation of the importance of the true solution of this problem. As early as 1598, Spain offered a reward of 1000 crowns for the discovery of a correct method of finding the longitude at sea. The States of Holland, at an early date, offered a reward of 100,000 florins, and France a reward of 100,000 livres. In 1714 the British government offered a reward of £10,000 to any one who should discover a method of finding the longitude at sea within sixty miles, £15,000 within forty-five miles, and £20,000 if within thirty miles. This offer did much to awaken interest in the subject. Though we have long

since passed the lowest limit then mentioned, thirty miles, it is doubtful if any two navigators will agree as to what limit we have actually reached. The general testimony of sea-captains in answer to my inquiries upon this point, is that one mile is the ordinary limit within which the co-ordinates of a ship's place can be determined. A few placed the limit at half a mile. Only one man placed the limit at five miles. He had had an experience of thirty-six years, and was considered a man of excellent judgment. For myself, I have great respect for his judgment.

I now propose to show that this is not an idle inquiry. The number of steam and sailing vessels belonging to the United Kingdom of England, Ireland and Scotland is about 25,000. The number belonging to the United States is at the present time about 22,000. These numbers vary from year to year with the conditions of trade and with the laws of supply and demand. A certain proportion are annually wrecked, either wholly or partially, the actual number for any given year varying to a certain extent with the total number of vessels, but being largely dependent upon the violence of the storms occurring during that year. But in a given series of years the average of the conditions of the latter class under which wrecks occur must be nearly the same, and the number of wrecks ought to be nearly proportional to the total number of vessels, under average conditions of carefulness in navigation. It is therefore essential to our investigation to inquire *whether the ratio of the number of wrecks to the total number of vessels in a given series of years is an increasing or a decreasing ratio.*

The following data are, for Great Britain, derived from the returns to the British Board of Trade, under the title of Wrecks and Casualties, and for the United States from the reports of the U. S. Life Saving Service. Inasmuch as the question of collisions does not necessarily come within the scope of the present investigation, the number of wrecks and casualties are given both including and excluding collisions.

GREAT BRITAIN.

Year.	No. Vessels (Steam and Sailing).	Total Tonnage.	No. of Wrecks including Collisions.	No. of Collisions.	No. of Wrecks excluding Collisions.
1856	25335	4304460	1153	316	837
1857	26219	4491377	1143	277	866
1858	26658	4587393	1170	301	869
1859	26804	4591250	1416	349	1067
1860	26764	4586742	1379	298	1081
1861	27142	4735491	1494	323	1171
1862	27525	4860191	1488	338	1150
1863	27750	5251757	1664	331	1333
1864	27737	5542878	1390	351	1039
1865	27868	5666873	1656	354	1302
1866	28072	5692010	1860	422	1438
1867	27918	5670350	2090	414	1676
1868	27635	5698774	1747	379	1368
1869	26389	5634727	2114	461	1653
1870	25643	5617693	1502	361	1141
1871	25188	5622660	1575	351	1224
1872	25093	5681963	1958	409	1549
1873	24873	5736368	1803	381	1422

UNITED STATES.

1873-4			1060	...	
1874-5			1610	...	
1875-6			2097	576	1521
1876-7			1935	521	1414
1877-8			1816	545	1271
1878-9			1953	568	1385

It is at once apparent from the returns for Great Britain that with a *decrease* in the number of vessels we have an *increase* in the number of wrecks and casualties. Comparing the data for the first half of the series with the data for the second half we have the following :

Limiting Dates.	Average No. of Vessels.	Average Tonnage.	Average No. of Wrecks in- cluding Collisions.	Average No. of Collisions.	Average No. of Wrecks ex- cluding Collisions.
1856-64	26882	4772449	1366	320	1046
1865-73	26520	5669046	1812	393	1419

Decrease in the number of vessels, 1 per cent.

Increase in the total tonnage, 19 per cent.

Increase in the number of wrecks including collisions, 33 per cent.

Increase in the number of collisions, 23 per cent.

Increase in the number of wrecks excluding collisions, 36 per cent.

The percentage of decrease for the number of vessels should really be considerably greater than appears from the table, since previous to 1865 the enumeration was limited to vessels of eight tons, while after 1865 all vessels of three tons and upwards were placed upon the register.

In the returns for the United States, the numbers for 1873-4 and 1874-5 are undoubtedly too small, as no adequate means were employed for collecting the data till after the congressional act of 1874. For the four years for which we have reliable data, the average number of wrecks excluding collisions is 1398, which in proportion to the number of vessels is somewhat less than the number for Great Britain during the years 1865-73.

Comparing the number of collisions which occurred in Great Britain with the number which occurred in the United States we find:

In Great Britain the ratio to the number of vessels is as 1 to 67.

In the United States the ratio to the number of vessels is as 1 to 43.

Since in a large number of cases the average of all the causes, except carelessness, which produce collisions must be a constant, the modulus of carelessness as between Great Britain and the United States may be stated to be as nearly as 2 to 3 in favor of the United States.

These comparisons seem to justify a new discussion of the whole problem of wrecks and their causes. The arrangements for collecting data respecting wrecks, under the auspices of the British Board of Trade, have been constantly improved since their inauguration in 1855. In the United States the improvement in this respect since the organization of the Life Saving Service in 1871, and especially since the more complete organization in 1875, is signally marked. In both countries, especially in Great Britain, the rules for the examination of both inferior and superior officers are rigidly enforced. In no other countries are courts of inquiry upon disasters more surely held or more thoroughly conducted, and yet with all these precautions it is almost certain that the average number of wrecks in any given series of years will exceed those for the same number of preceding years, the number of vessels remaining the same. It is hardly probable that unknown or unsuspected elements enter into this problem. It is possible, however, that the limits of official investigation may be enlarged with great advantage.

I shall presently attempt to ascertain the ratio of wrecks which occur from preventable causes. If I were asked to name the one cause

which, next to positive negligence, produced the largest number of wrecks of *this class*, I should say, *over-confidence*. This excess of confidence is, in general, inversely proportional to the intelligence of the navigator. Of course, a successful sea-captain *must* rely upon his judgment; but the inevitable tendency of many years of service, with almost every possible form of experience, is to convince him that his judgment is rarely in error. Especially is this the case in too positive forecasts of the weather, in the accuracy of his observations for position at sea, and in the performance of his chronometer. Of course he places the utmost confidence in the accuracy of all data derived from the Nautical Almanac. I shall presently show that even here this confidence is not justified. In 1860, Professor Main, in his address before the Royal Astronomical Society, said, "Hansen's Tables (of the moon) are practically perfect for all the purposes of navigation, and the great nautical problem of finding the longitude at sea is now completely solved." Is it not possible that assertions like this inspire a confidence which may be the cause of more wrecks than has been hitherto supposed? Is it a fact that the problem is completely solved? I assert distinctly that it is not. I know of no more mischievous and misleading statement than this by Mr. Main, unless it be that of our own countryman, Lieut. Maury, on the performance of the chronometers used in the Grinnel Arctic Exploring Expedition. In a letter to the makers, he says, "The instruments have been subjected to the severest tests to which it is possible to subject instruments of such delicate construction, yet so exquisitely were they provided with adjustments and compensations for the very great extreme of temperature to which they were subjected, that one of them, No. 114, Loseby, after having suffered all sorts of exposure to which such instruments are liable in a polar winter, is returned with a change in its daily rate during seventeen months of only .03 s." (Signed) M. F. Maury.

This may be called a first-class recommendation, and this endorsement being good by inference for all of Mr. Loseby's chronometers, purchasers are led to suppose that they are as near perfection as human skill can make them. By reference to the report of the annual trial of chronometers at the Greenwich observatory it appears that while Loseby made some very excellent chronometers, he made also some very poor ones. I think I am safe in saying that this is the experience with chronometers of every first-class maker.

Let us now see what is the actual state of the official inquiries into the causes of wrecks. By the Merchant's Shipping Act of 1854, it is assumed in the preliminary courts of inquiry, testimony which is held valid in Admiralty courts, that a vessel may be lost :

(a) By the act of God, *e. g.* being injured or destroyed by lightning, unavoidable perils of the sea, &c.

(b) By collision.

(c) By error of judgment, incompetency of master, or officers, or pilots.

(d) By bursting of boilers, breaking machinery, &c.

(e) By the removal of buoys, beacons, &c.

(f) By direct and wilful casting away.

(g) By inattention to necessary repairs.

(h) By defective charts.

(i) By not providing proper apparel and equipments for officers and crew.

(j) By improper loading or stowage.

(k) By neglect of regulations for preventing collisions.

(l) By employing a vessel in a trade for which she is unfit.

(m) By act of the Queen's enemies.

The nautical literature of this period is full of additional causes and remedies. The most important of these are :

(n) Failure to get the latitude.

(o) Failure to heave the lead.

(p) Unexpected deviation of compass.

One writer adds with a grain of truth, two other causes :

(q) A dead-and-alive set—no devil on board.

(r) Discord of dissension—the devil let loose.

An examination of the special and official inquiries into wrecks and casualties, and their causes, will show that the findings are based on one or more of these causes.

As an illustration, it appears from the Wreck Register for 1869 that there were during that year ninety-seven trials with the following results :

No. Cases	97
Acquittal from blame	39
Censures	10
Certificates cancelled, (drunkenness)	3
Certificate suspended, 5 years	1

Certificate suspended, 18 months.	.	.	.	2
“ “ 12 “	.	.	.	12
“ “ 9 “	.	.	.	2
“ “ 6 “	.	.	.	16
“ “ 3 “	.	.	.	4
“ “ 2 “	.	.	.	2
Left for decision of higher court	.	.	.	6
Total	.	.	.	97

It is my purpose in this investigation to examine only those cases of wrecks which in a measure seem to have escaped attention in official investigations. They are—

- I. Wrecks which occur from preventable causes.
- II. Wrecks resulting directly or indirectly from over-insurance.
- III. Wrecks caused by the deviation of the compass.
- IV. Wrecks caused by errors of observation at sea.

In attempting to distinguish between the causes of wrecks, one is compelled to acknowledge at the outset that the statistics reveal certain facts which in the present state of our knowledge are very difficult to explain. Apparently the most feasible way of ascertaining what proportion of wrecks occur from preventable causes, is to account for those which occur from all other causes which can be clearly assigned, and after making proper allowance for unknown causes, to place the balance in this class. But this cannot be safely done till further investigations are made with respect to certain facts which are revealed by the statistics already accumulated. These may be stated as follows:

(a) *The number of wrecks involving total loss is nearly proportional to the total number of vessels, while the number of wrecks involving partial loss shows a steady increase from year to year.* This will appear from the following table.

WRECKS OTHER THAN COLLISIONS IN THE UNITED KINGDOM.

Year.	Total Loss.	Partial Loss.	Year.	Total Loss.	Partial Loss.
1856	386	469	1867	356	1020
1857	384	482	1868	527	841
1858	351	515	1869	606	1047
1859	527	540	1870	411	730
1860	476	605	1871	398	826
1861	513	658	1872	439	1110
1862	455	695	Jan. to June		
1863	503	830	1873	212	522
1864	386	653	June to June		
1866	470	832	1873-4	346	1076

(b) *The increase in the number of wrecks outside of the "At Home" limits is proportionately much larger than within those limits. The "At Home" limits "embrace the cases which happen in waters within 10 miles from the shores of the United Kingdom; in waters within any bays or estuaries; in waters around any outlying sand-banks which are dry at low water; in the seas between Great Britain and Ireland; and between Orkney and Shetland and Western Islands and the mainland of Scotland."* Returns of this class began with 1867. I extract from the returns for 1873-4 the following significant numbers for British vessels only.

							Jan. to June.	June to June.
Total No. of wrecks,	1867	1868	1869	1870	1871	1872	1873	1873-4
casualties and collisions.	935	935	961	1208	1754	2415	1156	3094

These numbers include vessels belonging to the British possessions, but the total numbers belonging to this class follow nearly the same law as those which are referred to the United Kingdom; *i. e.* there is a steady increase till about 1866, and after that a steady decrease till 1874.

(c) *Over one-half of the total number of wrecks occur when the wind blows less than a fresh gale, or "when a ship if properly found, manned and navigated can keep the sea with safety."* The following table furnishes conclusive evidence on this point. It is taken from the Wreck Register for 1875.

WRECKS CLASSIFIED ACCORDING TO THE FORCE OF THE WIND.

1864 -65	1865 -66	1866 -67	1867 -68	1868 -69	1869 -70	1870 -71	1871 -72	1872 -73	1873 -74	Total.	
23	19	12	15	16	25	33	31	26	29	229	Calm.
18	26	27	22	31	32	57	42	57	48	360	Light air.
92	95	65	61	93	115	115	109	136	129	1010	Light breeze.
23	19	31	35	25	37	50	52	65	68	405	Gentle breeze.
149	150	176	150	157	162	161	198	175	211	1689	Moderate breeze.
210	228	228	199	185	208	180	213	248	234	2131	Fresh breeze.
175	187	230	196	250	205	234	255	312	233	2329	Strong breeze.
46	52	97	59	78	50	80	95	197	165	919	Moderate gale.
48	77	88	120	54	61	87	96	240	149	1020	Fresh gale.
414	605	617	417	629	515	336	234	348	205	4320	Strong gale.
211	157	204	310	139	186	182	126	246	160	1921	Whole gale.
37	46	28	62	29	34	29	21	56	31	373	Storm.
49	173	61	72	89	93	38	26	38	27	666	Hurricane.
9	2	1	2	—	—	5	8	8	22	57	Variable.
59	73	72	77	97	91	61	15	52	42	639	Unknown.

(d) *The number of wrecks is proportionately larger for new vessels than for those which have reached the average age of service.*

Data :

FOR GREAT BRITAIN.

Age, Limiting Years.	1865	1866	1867	1868	1869	1870	1871	1872	Jan. to June, 1873	1873-4	Total.
0- 3	151	277	209	176	198	130	155	211	94	182	1733
3- 7	260	280	322	297	406	307	302	374	209	354	3111
8-10	203	221	238	152	218	148	163	239	127	254	1963
11-14	173	198	262	268	308	218	198	242	136	227	2230
15-20	269	289	306	270	314	246	243	322	170	318	2747
21-30	352	419	441	383	436	272	311	344	163	297	3418
31-40	145	174	209	167	229	143	184	212	118	221	1802
41-50	84	87	118	100	112	63	81	86	61	81	873
51-60	51	53	60	35	53	49	44	47	19	41	452
61-70	20	25	36	28	18	20	19	24	13	13	216
71-80	9	13	9	9	9	4	6	17	8	6	90
81-90	5	5	3	8	5	5	8	4	2	5	50
91-100	1	2	1	..	2	..	..	5	..	2	13
101+	1	..	1	..	..	1	3	2	2	2	12
Unknown	288	296	298	238	286	259	210	252	84	188	2399

FOR THE UNITED STATES.

Age, Limiting Years.	1875-76	1876-77	1877-78	1878-79	Total.
0- 3	341	246	159	128	874
3- 7	318	276	368	305	1267
7-10	357	227	278	287	1149
10-14	275	243	324	307	1149
14-20	243	173	197	262	875
20-25	160	113	164	161	598
25-30	103	83	99	112	397
30-35	17	20	32	44	113
35-40	26	21	20	18	85
40-45	8	12	8	14	42
45-50	4	3	6	3	16
50+	7	1	4	7	19
Unknown	164	107	153	94	518

(e) *The number of wrecks is to a large extent dependent on the character of the cargoes of the vessels wrecked.*

Data :

FOR GREAT BRITAIN.

	Ballast [not Colliers].	Coal.	Colliers in ballast.	Fishing Snacks.	Grain and Provisions.	General Mdse.	Lumber.	Stone and Bricks.	All others.	Total.
1864-65	191	550	131	101	119	78	88	105	550	1913
1865-66	244	701	119	109	178	77	115	136	657	2336
1866-67	234	713	179	117	158	82	94	153	640	2370
1867-68	229	617	165	173	120	82	84	137	563	2170
1868-69	252	693	130	140	117	103	72	161	668	2306
1869-70	247	559	160	124	145	114	116	164	597	2226
1870-71	202	541	96	91	139	77	89	125	644	2004
1871-72	218	524	104	126	105	88	52	101	572	1890
1872-73	278	788	100	100	244	116	168	166	732	2692
1873-74	256	539	67	165	131	110	161	175	587	2191
Means	235	623	125	126	146	93	98	143	621	2210

FOR THE UNITED STATES.

1875-76	474	273		148	114	125	178	57	654	2023
1876-77	426	248		74	61	129	224	28	697	1887
1877-78	413	244		64	117	102	200	38	643	1821
1878-79	474	273		38	103	125	466	23	521	2023
Means	447	259		81	99	120	267	36	629	1938

After making a liberal allowance for the large number of vessels engaged in certain kinds of trade, it still remains that there is an excessive number of wrecks corresponding to certain classes of cargoes. I do not pretend to offer any explanation of the questions raised by a study of these tables. I can only call attention to the need of a careful study of the hidden causes of the anomalies which they reveal.

In attempting to ascertain the ratio of wrecks which occur from preventable causes, I am limited to two sources of information ; these are :

(a) The numerical data furnished by the Wreck Register for Great Britain, and by the reports of the Life Saving Service for the United States.

(b) The comparison of the findings of the courts of inquiry upon the causes of wrecks, casualties and collisions, ordered by the British Board of Trade or reported to this Board. Both in Great Britain and in the United States an attempt is made to distinguish the causes of all the wrecks which occur. In the British Wreck Register these causes are grouped in four classes as follows :

CLASS I.

- | | |
|---|---|
| 1. Foundering. | 6. Shifting of cargo or ballast. |
| 2. Driving or running on a sand
or lee shore. | 7. Missing stays. |
| 3. Parting cables. | 8. Failing to make harbor, or
stranding whilst entering. |
| 4. Dragging anchors. | 9. Capsizing. |
| 5. Damage to hull or rudder, or
loss of masts, yards, sails, &c. | |

CLASS II.

- | | |
|--|----------------------|
| 1. Error, neglect or incompetence of master or mate. | 3. Improper stowage. |
| 2. Error, neglect or incompetence of pilots. | 4. Not heaving lead. |

CLASS III.

- | | |
|--|--|
| 1. Unseaworthiness. | 4. Local attraction and defects of
compasses. |
| 2. Overloading. | |
| 3. Defective charts, insufficient
manning, unsound gear or
equipments, imperfect re-
pairs, or defective construc-
tion. | |

CLASS IV.

- | | |
|--|-------------------------------------|
| 1. Thick and foggy weather. | 7. Striking on sunken wreck, &c. |
| 2. Heavy seas. | 8. Spontaneous combustion. |
| 3. Strong currents and light
winds. | 9. Fire or lightning. |
| 4. Want of lights or buoys on
coasts and shoals. | 10. Damage to boilers or machinery. |
| 5. Want of pilot. | 11. Accident. |
| 6. Want of power in steam tugs,
or defective tow ropes. | 12. Combination of causes. |

In the reports of the Life Saving Service the causes are also grouped under four classes, but the arrangement is somewhat different from that given above.

In 1877, the two causes, "damage to machinery," and "explosion of boiler, bursting of steam pipes, etc.," were separated from Class IV and were grouped in a class by themselves. In this investigation the original grouping has been retained.

These classes are :

CLASS I.

- | | |
|-------------------|---|
| 1. Foundered. | 6. Damage to hull, rigging, rudder, &c. |
| 2. Stranded. | 7. Struck by lightning. |
| 3. Sprung a leak. | 8. Machinery disabled. |
| 4. Capsized. | 9. Miscellaneous. |
| 5. Waterlogged. | |

CLASS II.

- | | |
|-----------------------|------------------|
| 1. Error in judgment. | 4. Ignorance. |
| 2. Error of pilot. | 5. Carelessness. |
| 3. Neglect of master. | |

CLASS III.

- | | |
|---------------------------|---------------------------------|
| 1. Defective instruments. | 2. Defective hull, rigging, &c. |
|---------------------------|---------------------------------|

CLASS IV.

- | | |
|--------------------------------|-------------------------------|
| 1. Adverse currents. | 12. Waterlogged. |
| 2. Heavy sea. | 13. Explosion. |
| 3. Accidental. | 14. Absence of proper lights. |
| 4. Fire. | 15. Miscellaneous. |
| 5. Never heard from. | 16. Ice. |
| 6. Thick and foggy weather. | 17. Machinery disabled. |
| 7. Misstayed. | 18. High wind. |
| 8. Sprung a leak. | 19. Darkness. |
| 9. Becalmed. | 20. Tides. |
| 10. Parted chains, &c. | 21. Unknown. |
| 11. Struck bridges, piers, &c. | |

Under the subdivisions given above we have the following data :

FOR GREAT BRITAIN.

WRECKS AND CASUALTIES OTHER THAN COLLISIONS FOR 10 YEARS.

Year.	Wrecks resulting in Total Loss arising from					Casualties resulting in Partial Dam- age, arising from					Total.
	Class I.	Class II.	Class III.	Class IV.	Unknown.	Class I.	Class II.	Class III.	Class IV.	Unknown.	
1865-6	320	120	62	64	19	574	127	47	143	11	1487
1866-7	340	121	74	72	15	538	120	65	161	10	1516
1867-8	269	85	55	87	20	571	130	70	134	6	1427
1868-9	276	94	95	96	17	470	151	84	160	8	1451
1869-70	253	66	56	89	36	557	134	65	150	6	1412
1870-1	203	89	58	97	26	375	159	102	176	8	1293
1871-2	72	104	43	78	13	341	199	144	158	5	1157
1872-3	185	95	52	97	67	632	223	133	244	4	1732
1873-4	128	93	30	53	42	525	180	91	251	29	1422
1874-5*	177	84	33	75	42	{ 393† 906‡	182 234	129 59	240 325	37 15	2931
Total for } 10 years }	2223	1509		1105		5882	2828		2281		15828

* In this year the partial damage is subdivided into † serious and ‡ minor casualties.

FOR THE UNITED STATES.

Year.	Class I.	Class II.	Class III.	Class IV.
1865-6	804	82	35	524
1866-7	706	50	33	543
1867-8	940	20	55	261
1868-9	990	18	74	336
Means	860	42	49	416

From these figures it is impossible to draw other than very general conclusions. Since the causes enumerated in Class IV are about equally divided between those which may be classed as preventable and those which are non-preventable, we may with tolerable confidence compare Classes I and II.

It appears therefore that for the total number of wrecks excluding collisions, 45 per cent are due to preventable causes.

For the United States I can offer no explanation of the extremely small ratio of Class II to Class I. The omission of the cause "failure to heave the lead," suggests that the data is not altogether reliable. That this cause cannot be safely omitted will appear from the fact that between 1856 and 1872, out of less than five hundred cases of punishment imposed for default by English courts of inquiry, 102 were for "neglect of lead."

In 1865 the British Board of Trade began the yearly publication of a resumé of the findings of the courts of inquiry instituted for ascertaining the causes of wrecks, and for the punishment of those convicted of default in navigation, by reprimand, by suspension or cancellation of certificates of competency, and even by fines and imprisonments. In the Wreck Register for 1873 there is a brief resumé of all the trials which occurred between 1856 and 1873. The total number is 1163. I have examined each of these findings, and selected those in which there is a clear acquittal from blame, care being taken to exclude all cases in which there is even an admonition, although the certificate was returned. From this examination it appears that there were 384 cases out of the total number 1163 in which the casualties occurred from causes clearly beyond the control of the master or mate.

From an examination of the findings between 1867 and 1874 I find the following results:

Year.	No. of Acquittals.	Total No. of trials.	Year.	No. of Acquittals.	Total No. of trials.
1865	29	46	1870	86	251
1866	46	81	1871	44	130
1867	9	30	1872	70	169
1868	21	47	1873	38	102
1869	39	97	1874	122	314

We have here 504 clear acquittals in 1267 trials, giving a ratio not far different from that obtained from the findings between 1856 and 1872, in which as nearly as can be made out the cases tried were different from those recorded in the annual volumes.

It is not to be understood that the findings in the remaining 763 trials are all for default of the master or mate, inasmuch as the inquiries often extend to their conduct *after* the casualties occurred.

In the Register for 1876 there is a remarkable increase of the number of trials reported, covering the years 1874-5 and 1875-6. I copy the summary given:

Cancellation of certificates		11
Suspension of certificates		204
Certificates returned		178
Unseaworthiness		13
Overloading, bad stowage		18
Defects in equipments		7
Stress of weather		128
Fire or other accidents		68
Cause not stated		64
Total		691

In both of these investigations there is one class of wrecks for which no cause can be safely assigned, viz. that involving vessels which were never heard from after sailing, for in these cases there are no records either of efforts made or of sufferings endured. The reports with respect to this class of wrecks are very defective. In the Wreck Register for 1874 there is for the first time a separate list of British ships not heard from after sailing. Of this class there were 83 in 1873-4, 137 in 1874-5, and 101 in 1875-6. The total number reported in the reports of the Life Saving Service for the years 1875-6 to 1878-9 is 59. It is probable that these numbers are somewhat too small, for according to the Insurance Reports of Great Britain there were between 1864 and 1869 (inclusive ?) 10,588 wrecks of which the end of 846 is unknown.

There is also another class of wrecks which may be much larger than is usually supposed, viz. wrecks for which it is exceedingly difficult to determine the true cause. In any given investigation it is imperative that the inquiry shall be extended beyond the immediate cause. A wreck may apparently occur by the act of God and yet be due to the previous folly of man. Take the case of the barque Providence, which sprung a leak in the Baltic on a voyage to Dantzic. After the crew had arrived at the last point of exhaustion in their efforts to stop the leak, it suddenly stopped, and the vessel arrived safely at her destination. After the discharge of the cargo it was found that the leak had been caused by the giving way of a knot in a bottom plank, and that it had been stopped by a dead fish, which acted as a plug. If the vessel had been lost there would have been no evidence of the cause. As it turned out, the builder had to suffer the penalty for what might have been charged with apparent reason

as an act of God. One can hardly tell where to place the case of the steamship *L'Amerique* of the French line between New York and Havre. She sprang a leak, and after being abandoned, was picked up and manned by a British crew, when it was found that through the ignorance of the engineer the donkey engine had been pumping water *into* instead of out of the ship.

Combining the results obtained from these independent inquiries, the general conclusion is reached that about one-half of all the wrecks which occur may be due to preventable causes.

Under the second head a complete and satisfactory investigation is wellnigh impossible. That the whole question of insurance in connection with wrecks involves more or less of fraud is evident from the following extract of a speech before the United Service Institution by Admiral Halstead, Secretary of Lloyds. He said: "The remedy for shipwrecks—what is it? I do not pretend for one instant to be able to provide a remedy, and I do not know anybody who can undertake to say what is a remedy; but I will tell you this: if I could go on the Stock exchange to-morrow morning, and by holding up my hand put a stop to all shipwrecks upon the coast, it would be a question how I could get safe with life off that Exchange. When I put that question to him (Lloyd) he said, '*It is perfectly true, you would stop our bread.*'"

As for definite data, it is only possible from the nature of the case to obtain a little here and a little there. The cases of the *Dryad* and the *Harlequin*, in 1837, show that in those days at least the question of insurance had a very definite bearing upon the question of wrecks.

In 1866, Thomas Berwick was connected as an accessory in the destruction of ships owned by Thos. Berwick & Son. On his trial he confessed to having destroyed no less than nine vessels in the course of twenty years.

In 1867 there were in the Baltic 215 British vessels, and in 1868, 220 Swedish. The British vessels were largely insured, while on the Swedish vessels there was very little, if any insurance. Of these numbers, 17 British and 3 Swedish were lost. From 1857 to 1867 the ratio was 10 British to 3 Swedish. It is hardly probable that this disparity of numbers can be accounted for by the superior skill of the Swedish navigators.

The data which I shall now offer will need to be taken with a large allowance for imperfect returns, but the near approach of the total insurance to the total value of the vessels and cargoes lost, and espe-

cially the large excess of the insurance over the actual loss, proves either that the business of insurance involves enormous profits, or that it involves extensive fraud on the part of those who insure. As before, the returns are derived from the Wreck Register for 1875, and from the annual reports of the Life Saving Service between 1875-6 and 1878-9.

FOR GREAT BRITAIN.

VESSELS.								
Year.	Total No. Insured.	Total Am't of Insurance	No rp'd not Insur'd.	Unknown.	No. on which loss was report'd	Estimated loss.	Av'g am't of Ins. on each ves-sel.	Average loss on each ves-sel.
1865	811	£1,102,070	298	903	1331	£1,090,568	£1359	£797
1866	894	1,250,262	416	979	1854	1,018,000	1399	549
1867	1072	1,380,912	481	960	2067	1,282,888	1288	621
1868	905	1,195,025	456	769	1727	899,604	1317	522
1869	1114	1,758,350	478	1002	2102	1,240,535	1578	590
1870	711	1,474,414	356	798	1483	964,317	2074	650
1871	800	1,623,099	419	708	1565	1,141,845	2029	730
1872	1009	2,092,830	477	895	1950	1,165,466	2074	598
Jan-Jun 1873	500	1,265,743	227	479	1006	582,489	2531	579
1873-4	847	1,930,907	502	842	1842	899,201	2280	488
1874-5	1597	4,280,813	893	1769	3347	1,369,709	2681	409

CARGOES.

1865	108	£115,905	135	1312	468	£500,467	£1073	£1069
1866	160	376,869	167	1512	590	453,640	2355	769
1867	212	300,117	228	1413	637	625,602	1416	982
1868	183	299,985	202	1276	529	366,697	1639	688
1869	284	487,506	226	1498	642	345,843	1717	539
1870	82	482,348	213	1135	486	414,510	2650	853
1871	201	703,262	257	1086	475	372,772	3499	785
1872	282	476,448	363	1374	536	428,402	1690	799
Jan-Jun 1873	141	248,282	114	723	284	296,667	1761	1045
1873-4	250	324,806	226	1227	406	289,096	1399	712
1874-5	311	807,200	67	2380	495	334,054	2595	675

FOR THE UNITED STATES.

VESSELS.

Year.	Total No.	Total Value.	Total No.	Total Loss.	Total No.	Total Am't Insured.	Average value of each vessel.	Average loss on each vessel.	Average Ins. on each vessel.
1875-6	1800	\$33,839,675	1788	\$7,025,192	807	\$11,677,106	\$18,000	\$3029	\$14,470
1876-7	1771	35,973,839	1709	8,834,202	782	10,258,611	20,313	5169	13,118
1877-8	1724	29,195,845	1640	6,963,084	748	8,681,180	16,936	4246	11,606
1878-9	1853	29,450,662	1749	5,899,159	733	7,337,011	15,893	3373	10,012

CARGOES.

1875-6	1307	\$17,333,636	586	\$2,687,228	487	\$ 8,589,516	\$13,292	\$4586	\$17,638
1876-7	1283	21,230,636	612	4,548,958	598	11,882,253	16,548	7433	19,870
1877-8	1276	18,738,387	621	3,979,806	562	9,678,110	14,685	6409	17,221
1878-9	1343	16,890,435	630	4,284,836	63	7,751,431	12,577	6801	13,768

Under the third heading my investigations are far from complete or satisfactory, on account of the difficulty of obtaining reliable data. The compass problem is an intricate one, and has never been fully solved, though the researches of Flinders, Barlow, Scoresby, Airy, and Harkness have done much to convert great uncertainty into tolerable certainty. The discovery of the deviation of the compass is undoubtedly due to Columbus, though the claim is sometimes put forward in behalf of one Peter Adsiger, based on a manuscript deposited in the University of Leyden. But Humboldt has shown that this claim is a spurious one. The first observations on the variation were by Bond, in 1668. But the variation is a very irregular one. There are both yearly, monthly, and diurnal inequalities, the diurnal variation being discovered by Graham in 1722. (The dip of the needle is said to have been discovered by Robert Norman in 1576.) The following short table of variations at Greenwich will illustrate both their magnitude and their irregularity.

Year.	Mean variation Greenwich.	Range during year.
1865	20° 32' 43"	3'
1866	20 27 47	11
1867	20 20 17	7
1868	20 13 14	6
1869		0

As an illustration of the monthly irregularity of variation, I give the Greenwich record for the first six months of 1841:

Month.	Variation.	Change from one month to next.
Jan.	23° 11' 46"	+5' 50"
Feb.	17 36	+1 42
Mar.	19 18	-7 30
April	11 48	+5 54
May	17 42	-1 24
June	16 18	

Here we have variations between different months amounting to nearly one-fourth of a degree. But the fluctuations of a single day are often more than for a whole month. These diurnal fluctuations are well illustrated by the following curve taken from the Greenwich observations of 1869.

The variation for long periods is well illustrated by comparing Halley's Chart for 1700 with modern charts. But the complexity of the problem does not stop here.



The tendency of the present time is to build iron ships, and all ships now have more or less iron in their construction. These ships become to a greater or less degree themselves great magnets. In wholly iron ships the uncorrected deviation of the needle often amounts to 50° , thus rendering it wholly useless. Captain Flinders in his voyage to Australia in 1811 was the first to make an intelligent discussion of this subject. The result of his investigations may be stated as follows :

I. A compass gives different bearings of the same object when placed in different parts of the ship.

II. When the ship's head is on the magnet meridian there is no effect from local attraction, showing that the various masses of iron on board act in unison with the earth's magnetism.

III. When the ship's head is east or west the effect of local attraction is the greatest, and that at intermediate points the deviation varies as the sine of the angular distance between the magnetic meridian and the bearing of the ship's head.

IV. The maximum variation is different in different parts of the world, varying as the distance from the magnetic equator.

The polar expeditions of 1818 fully confirmed Flinders' experiments. Before reaching Greenland the compass of the *Alexandria* differed from that of the *Isabella* 11° , and the same compasses gave results differing 11° when placed in different parts of the ship. As the vessels passed up Davis straits all the compasses became sluggish. In Sir Edward Parry's passage through Barrow strait his compasses became totally useless. The admiralty law in regard to swinging for the variation of the compass is a very clear statement of the case. It reads as follows :

"As the deviation or error of the compass caused by local attraction of the ship becomes changed in amount by any change in the ship's geographical position, and may be entirely reversed in its direction by the ship's proceeding from the northern to the southern hemisphere, it is to be invariably tested by azimuth and amplitude observations at sea, and the ship is to be swung for ascertaining the change of error on arrival at a foreign station, and also once each year, and the same is to be inserted in the log-book and sent to the Admiralty with the quarterly return for December."

The next important discovery in this connection was by Barlow, who found that all the influence of iron bodies exerted on the compass resides on the surface. This discovery paved the way for Airy's

method of correcting compasses, which is by swinging the ship in the usual way, and then correcting the local attraction of the ship by means of permanent magnets of soft iron conveniently placed with respect to the compass. But the most important discovery was made by Dr. Scoresby. He found that every iron ship is itself a magnet, and that it gets its magnetism while building by the inductive magnetism of the earth, the poles of the ship's magnetism depending on the position of the building-yard and the direction of the keel in construction. Dr. Scoresby made the voyage of the world in the Royal Charter to test his theory, found by induction, and found it fully confirmed. Before starting his compasses were corrected by Airy's plan. On arriving at Melbourne it was found that a complete inversion of the ship's magnetic polarity had taken place. Every stanchion, every standard, every davit, every mass of iron about the deck had in its upper surfaces acquired a northern instead of a southern polarity, and the starboard compass had lost nearly one-half of its original errors. On returning to the place of starting in the northern hemisphere, and swinging the ship, it was found that a re-inversion had taken place; but the compasses did not quite return to their original deviations, but retained a fraction of their errors. It has since been found that these changes are much greater in steam than in sailing vessels, as shown by observations on board the Vulcan (steam) and the Pandora (sailing).

In 1852-3 Dr. Scoresby, in a paper before the British Association, showed that there is a sensible difference in the deviation before and after steam is up. It is said that the compasses of a steam vessel when light and running before the wind with a high sea are practically useless.

Within a few years the effect of the heel of a ship on the deviation of the compass has been pretty fully investigated. In general it is found that when a ship's head is nearly E or W there is no sensible effect from heeling, that when it is N or S the effect is greatest, and that the marked end of the needle is attracted to the raised side of the ship in north latitudes, and *vice versa* in south latitudes. In iron ships 1° of heel produces from 1° to 2° variation of the compass, generally about 1° . In a voyage of the City of Sydney, iron, 1100 tons, the deviation amounted to $2\frac{1}{2}^\circ$ for every degree of heel. This deviation may be almost wholly prevented by the use of elevated compasses.

More recent experience has shown that the magnetism of an iron ship does not attain its normal condition till some twelve months

after launching, and that for some time after the variation is very irregular. In the *Great Eastern*, a fixed compass changed its deviation nearly three points in the first nine months of service. The observations of Professor Harkness on board a monitor seem to be conclusive on this point. It is apparent from this brief review that even with the utmost caution, ships may be misled by unknown variations, and yet the London Compass Committee as late as 1869 declare that very few ships are lost from this cause. What shall be said of ships that are never swung and whose masters know nothing of the laws of variation? The loss of the *City of Washington* is the best refutation of this statement. One of the most intelligent captains I ever met, a man who has commanded vessels for thirty-six years, and who has been to almost every great port on the surface of the globe, told me he had never swung his ship and that he had never seen a ship swung.

The opinion of the London committee was probably founded on the small number of disasters arising from errors of the compass reported in the annual returns. Between 1865 and 1874-5 only 34 cases are set down as due to this cause, while in the findings of the courts of inquiry between 1856 and 1872 there are only 21 cases of this class. In the U. S. Reports there are recorded 20 cases between 1875-6 and 1878-9, but here the distinction is not always made between "error of compass" and "error of chronometer."

That other competent authorities hold to a view radically different from that expressed by the London committee will appear from the correspondence between the Royal Society and the Board of Trade, printed in the *Shipping Returns for 1865* (vol. LXV, 1866). The correspondence opens with the following letter:

ROYAL SOCIETY TO THE PRESIDENT OF THE BOARD OF TRADE.

THE ROYAL SOCIETY; BURLINGTON HOUSE, 25 May, 1865.

Sir:—The attention of the Fellows of the Royal Society has been recently directed to the very great increase which has taken place in the employment of iron in the construction and equipment of ships, and the consequent augmentation of the embarrassments occasioned in their navigation by the action of the ship's magnetism on their compasses.

The inconveniences which have already made themselves felt in the ships of the mercantile marine, and which threaten to be productive of very serious loss of life and property, unless remedial measures be adopted similar to those which have proved so advantageous to the ships of Her Majesty's navy, have induced the President and Council of the Royal Society, after much considera-

tion, to venture on the step of calling your attention, as presiding over the Department of Trade, to a subject which they believe to be of pressing importance.

In this view, the accompanying memorandum has been prepared, stating as briefly as may be, the particulars which they are desirous of bringing under your consideration, in the belief that the time has fully arrived when measures of a more stringent and effectual character are required in the direction which has been already taken by Her Majesty's government in such legislative enactments as those contained in the "Merchant Shipping Act, 1854," adverted to in the accompanying memorandum.

I have only to add, that it would afford the President and Council great satisfaction if they could be of any further assistance in a matter which they believe to be of so much importance.

I have, &c.,
EDWARD SABINE,
President of the Royal Society.

In the memorandum referred to, attention is called—

1. To the great increase in the number of iron ships, as well as in the amount of iron used in the construction of such ships.
2. To the losses of iron ships.
3. To the advances which have been made in, and the present state of the science of the deviation of the compass.

Also, the attention of the Board of Trade is drawn to the proposition to secure under competent authority—

- (a) The correction of the compass in particular ships.
- (b) The advancement of the science of the deviation of the compass.
- (c) The education of masters and mates.

The reply of the Board of Trade to this communication contains a large amount of useful information, but fails, it seems to me, to grasp the vital point at issue, viz. the application to the mercantile marine of the system which has proved eminently satisfactory in the Royal navy.

The correspondence on the part of the Royal Society closes with the following communication:

THE ROYAL SOCIETY TO BOARD OF TRADE.

BURLINGTON HOUSE, 2d November, 1865.

Sir:—I have now laid before the Council of the Royal Society, your letter of the 25th of July, referring to the adjustment of the compasses of iron ships, and a copy of my letter of the 28th of August, acknowledging its receipt and advert-

ing to the inquiry you had made as to the preparation of a "manual" on the subject, together with your subsequent letter of 23d October having reference to the same inquiry.

The President and Council are much disappointed by learning that the Board of Trade are not prepared to give effect to the recommendation that the system which has been found to work so successfully in the Royal navy, of combining official and competent superintendence with a proper code of instruction, should be extended to the mercantile marine. They consider such superintendence to be essential, not only to the general introduction of a good and efficient mode of compass correction into the mercantile marine, but even to the discharge of the duties having respect to the adjustment of the compasses of sea-going passenger steamers, with which the Board of Trade is already charged by the legislature.

In the memorandum accompanying my letter of the 25th of May it was stated that many recent losses of iron steamers have taken place, in which it is probable that compass errors have occasioned the loss. The President and Council think it right to call the attention of the Board of Trade to the serious responsibility they incur in cases of loss of life and property arising from the want of proper system of compass adjustment, by declining to take the course which is pointed out by the concurrent opinion of all competent advisers, as not only the best, but the only method of securing the introduction of such a system. They cannot but look forward to a time when the necessity of a proper supervision will be forced on the executive by public feeling, excited by some disastrous loss of human life traceable to the want of such superintendence.

The question is one of such vital importance that they desire to submit to the consideration of the Board of Trade the accompanying memorandum, replying in some detail to passages in your letter of 25th July.

I have, &c.,

(Signed)

EDWARD SABINE,
President of the Royal Society.

As far as I can learn the compass problem still remains in the unsatisfactory state indicated by this correspondence.

[*To be continued.*]

THE PROCEEDINGS

OF THE

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Vol. VII. No. 4.

1882.

Whole No. 18.

NAVAL INSTITUTE, ANNAPOLIS, MD.

JANUARY 28, 1881.

LIEUT.-COMMANDER P. F. HARRINGTON, U. S. N., in the Chair.

THE COEFFICIENT OF SAFETY IN NAVIGATION.

(*Concluded.*)*

BY PROF. W. A. ROGERS, of CAMBRIDGE (U. S.) OBSERVATORY.

I now enter upon the consideration of the fourth topic. As has been already stated, the British government in 1714 offered a reward of £20,000 to any one who should discover the longitude at sea within thirty miles.

Two methods were proposed for the solution of the problem.

Morin proposed what is now substantially the lunar method; and Maskelyne devoted all his energies to the solution of the problem by observing astronomical phenomena, such as the eclipses of Jupiter's satellites. Maskelyne made a trial of this method during a voyage to Barbadoes, with an apparatus suspended from the rigging of a ship, called Irwin's marine chair. But the attempt was not successful, owing to errors occasioned by the motion of the ship.

On the other hand, mechanicians devoted every energy to the mechanical solution. Huygens had, as early as 1665, in a voyage to the coast of Guinea, made the trial of the method by watches, without success. Trial of this method was now renewed with watches made

* See p. 205 *et seq.*, No. 3, Vol. VII, Proceedings Naval Institute.

by this maker, but it was soon found that the method would be of little value till some contrivance was devised for correcting errors produced by the variation of the temperature. At last Harrison, by the very perfection of his workmanship, produced a chronometer with which a trial of the coveted prize was made in a voyage to Jamaica. As the longitude of the island differed from that shown by his chronometer by only ten and three-quarter miles, he claimed the reward, which, after a successful second trial, was awarded to him.

We have, then, two essentially different methods for the determination of the longitude at sea :

(a) By lunar distances, occultations, and eclipses of Jupiter's satellites, &c.

(b) By chronometers, assuming a constant rate throughout the entire voyage.

The latter method has for a long time been regarded as far more reliable than the former. Let us examine into the grounds of this opinion. We are, at the outset, met with the difficulty that in observations at sea we have no means of comparison with the truth. It is not sufficient to say that several observations agree with each other. Agreement *inter se* may be quite a different thing from agreement with the truth. We are obliged to have recourse to observations made on land, and then find, if possible, some means of comparing the value of these with those made at sea.

Even in the determination of the positions of fixed observatories, in which appliances for the utmost refinement are at hand, the values derived often vary widely from the truth. As late as 1755, a century after the establishment of the observatory at Greenwich, the difference of longitude between that station and the Paris observatory was assumed to be 9m. 16s. Gen. Roy, in the progress of the trigonometrical survey of England, obtained the value 9m. 18.8s. In 1796 9m. 20s. was assumed to be the correct value. In 1830 it was found to be 9m. 21.5s. from one thousand transits of moon and stars, while the actual difference, as determined by the telegraphic method, was found to be 9m. 20.6s. We have here a variation of 5.5s., or one and one-quarter miles.

The range of the earlier determinations of the difference between the longitude of Greenwich and Brussels is ten miles. A thorough discussion from moon culminations in 1836 gave a result which differs from that since found by the telegraphic method over 1s. To indicate the magnitude of the errors of longitude of the old State House,

Boston, from transits of planets over the sun and from eclipses, I give the results of the several determinations by Dr. Bowditch:

	State House from Greenwich.	Greenwich from Harvard Col'ge.	Deviation f'm truth.
	<i>h. m. s.</i>	<i>h. m. s.</i>	<i>s.</i>
From transit of Mercury....1743	4 44 19.2	4 44 31.6	— 0.7
Eclipse of the sun.....1766	17.5	29.9	+ 1.0
From transit of Venus 1769	18.0	30.4	+ 0.5
Eclipse of the sun.....1778	14.9	27.3	+ 3.6
“ “ “ 1791	18.0	30.4	+ 0.5
“ “ “ 1806	11.8	24.2	+ 6.7

We have here an absolute error of 2.2s., or more than one-half a mile, with a range of 10.3s. In passing, I may say that these results are a remarkable approximation to the true value, but they were deduced by one of the greatest and most practical mathematicians then living. They are, however, hardly comparable with any single observation, either on land or sea. During the time it would take an ordinary computer to “work up” these observations, a fast-sailing ship would be well on her journey around the world.

Perhaps the best illustration of the uncertainty resting upon the early determinations of longitude is furnished by the various values of the longitude of Washington, which have been given.

In 1822 Lambert found for the longitude of the Capitol :

	<i>h.</i>	<i>m.</i>	<i>s.</i>
Occultations in 1793..... $\lambda =$	5	7	5.2
“ “ 1804.....		7	37.7
Eclipse in 1811.....		8	21.8
“ “ 1813.....		7	43.5.
Value adopted.....	5	7	42.0

Elliott in the same year found $\lambda = 5\text{h. } 8\text{m. } 7.2\text{s.}$, but his value was rejected from the mean given above, on account of its supposed discordance. We have here a range of over nineteen miles, and the final value is nearly six miles in error.

Even in the determination of the longitude of the present observatory there is comparatively a wide range between the different results.

Adopting $\lambda = 5\text{h. } 8\text{m. } 12.1\text{s.}$, we have:

				Value Found.	Correction.
				<i>h. m. s.</i>	<i>s.</i>
Bond Chronometer expedition.....	1849-50			5 8 12.0	+ 0.1
" " "	1851			12.3	— 0.2
" " "	1855			13.4	— 1.3
From moon culminations	{	Loomis.....		5 8 7.4	+ 4.7
		Gillis		10.0	+ 2.1
		Bache		9.9	+ 2.2
		Newcomb..1860		11.6	+ 0.5
		Newcomb..1862		9.8	+ 2.3
Eclipses.....	{	Bache.....		11.1	+ 1.0
		Pierce.....		11.6	+ 0.5
Occultation of Pleiades....		Pierce.....		11.4	+ 0.7

We have here a range of 6s., or one and one-half miles, while the mean is in error 1.3s.

Two hundred and six moon culminations gave a longitude for San Francisco which has since been found to be 4s. in error.

The telegraphic determination of the longitude of Lisbon, by Lt. Comdr. F. M. Green, U. S. N., giving a result 8s. different from the accepted value, is the most recent instance of an erroneous value.

These examples are sufficient to show that it is no easy task to determine accurately the geographical position of a point on the earth's surface, even under the most favorable conditions and with the most perfect instruments.

Resuming our investigation, we begin with moon culminations. These, though essentially the same in principle, are probably somewhat more accurate than lunars. With the exception of the Willet's Point observations of lunars, we are obliged to limit our investigation to observations of nearly the same class, viz. moon culminations. These are more accurate when the longitude depends upon observations at both of the stations to be determined, since the errors of the tables of the moon are thus for the most part eliminated.

In the case of a fixed observatory whose position has been ascertained by other and more accurate methods, we have the data for testing the accuracy of this method. Before the application of the telegraphic method of determining longitude, many observations of this class were made. I have collected and reduced the most important of these with the following results. All the observations made during a given year were grouped according to the limb of the moon observed. The number of observations for each limb and the number of years during which the observations were continued are given in the following table:

Places of observations.	1st Limb.	No. of	2d Limb.	No. of
	Limiting years.	observations.	Limiting years.	observations.
Washington .. Edinburgh...	1839-41	67	1839-41	23
Washington .. Greenwich...	1839-42	61	1839-42	42
Washington .. Hamburg....	1839-42	65	1839-42	12
Washington .. Oxford	1840-41	39	1840-41	12
Washington .. Cambridge, En.	1839-42	58	1839-42	33
Greenwich .. Hudson.....	1839-42	57	1839-40	16
Cambridge .. Hudson.....	1839-44	74	1839-42	..
Edinburgh .. Hudson.....	1838-43	86	1840-42	15
Oxford .. Hudson.....	1840-43	30	1840-	5
Hamburg .. Hudson.....	1839-44	58	1842-43	19

Omitting all details, the following results were obtained from these investigations:

Washington (Old Observatory) and Greenwich.

$$\lambda = \begin{matrix} h. & m. & s. \\ 5 & 8 & 2.2 \end{matrix}$$

	Limb of moon.	No. of obs.	Mean Corr.	Extreme range in each year.
			s.	s.
Washington .. Greenwich.....	I	60	-1.3	
	II	41	-1.0	23.0
Washington .. Greenwich via Edinburgh ..	I	64	-5.1	
	II	23	-0.8	21.8
Washington .. Greenwich via Oxford....	I	32	-5.4	
	II	13	-0.4	21.6
Washington .. Greenwich via Cambridge	I	57	+0.8	
	II	23	-4.6	19.9
Washington .. Greenwich via Hamburg..	I	69	-2.6	
	II	12	-2.5	21.6

Washington and Edinburgh.

Observations at each Station.

No. observations.	Average deviation from mean for each year.	Extreme range between different years.	Coefficient.
...	2.4s.	21.8s.	3.6

Camp Riley, Boundary between United States and Mexico.

From tabular places of the moon. Comparison with mean value.

	s.	s.	
15	8.3	35.3	4.3

Hudson, Ohio.

Comparison with the mean value of series.

	No. obs.	Average deviation from mean each year.	Extreme range between different years.	Coefficient.
		s.	s.	
Hudson and Greenwich.	73	5.0	20.2	4.0
Hudson via Cambridge..	74	5.2	18.2	3.5
“ “ Edinburgh..	101	7.1	17.1	2.4
“ “ Oxford.....	35	7.6	13.6	1.8
“ “ Hamburg...	77	6.7	18.0	2.7

Brussels and Greenwich.

		s.	s.	
Brussels and Greenwich..	17	6.1	14.3	2.3
“ via Cambridge..	15	3.2	11.7	3.7
“ “ Edinburgh..	17	7.0	27.6	3.9
“ “ Altona.....	7	3.8	16.6	4.4

F. G. W. Struve in Turkey with 22-inch Transit.

Comparison with mean value.

		s.	s.	
Paris .. Jassy.....	11	8.2	36.9	4.5
“ .. Roman	7	4.8	21.5	4.5
“ .. Busen	11	7.4	29.2	4.0
“ .. Kolarsch.....	7	5.0	18.7	3.7
“ .. Schmerscha.....	8	3.5	16.2	4.6
“ .. Kalafat.....	10	8.4	39.2	4.7
“ .. Warna.....	18	9.7	41.5	4.3
“ .. Tiflis	12	5.7	21.2	3.7
“ .. Erzerin	18	7.8	25.9	3.3

WILLET'S POINT.

Comparison with transit observations.

From moon culminations.

			s.	s.	
	1869	18	12.6	64.4	5.1
American..	1870	17	14.5	67.5	4.7
Ephemeris.	1871	12	7.2	31.3	4.4
	1872	6	2.9	9.0	3.1
	1869	18	12.5	47.9	3.8
British	1870	18	16.6	71.0	4.3
Ephemeris.	1871	13	17.7	76.9	4.3
	1872	6	8.9	24.4	2.7

From lunar distances.

		<i>s.</i>	<i>s.</i>	
1870	..	34.9	67.4	1.9
1871	..	37.8	143.0	3.8
1872	..	49.5	80.9	1.6

As an illustration of the error of Mr. Main's statement that the lunar problem is completely solved, I add the details from the Willet's Point observations for 1871:

From Moon Culminations.

Date.	Corr. by British Ephemeris.	Corr. by American Ephemeris.
January 30	— 5.2	— 17.1
September 25	— 24.2	— 9.1
“ 26	+ 14.9	+ 1.6
“ 27	+ 11.0	+ 1.4
“ 27	+ 2.2	— 8.4
“ 28	— 2.9	..
“ 28	— 16.2	— 12.1
“ 29	— 10.1	— 10.0
October 23	— 14.6	+ 11.9
“ 27	+ 7.8	+ 0.9
“ 27	+ 18.9	+ 14.2
“ 28	+ 50.2	+ 0.0
“ 28	+ 52.7	+ 0.1

From Lunar distances for 1870.

Date.	With Sextant.	Corr.
January 27		+ 19.1
September 3		+ 47.0
October 19		— 18.4
“ 29		— 1.0
“ 30		— 0.2
November 1		— 38.0
“ 6		— 96.0
“ 29		— 82.3

Finally, we have fortunately on this point positive and conclusive testimony from the observations of Mr. Fisher, astronomer on Capt. Parry's second voyage. He found that the mean of 2500 observations in December differed ten miles from the mean of 2500 in the following March, and that the mean of a still larger number made on both ships differed ten miles from those in March and twenty-four miles from those in December. It is the testimony of Capt. Heywood that any set of lunars may be expected to differ 6' or 7' from any other

set equally good, taken at a different time of the year, and this independently of accidental errors.

Collecting results, we have :

Place and Circumstances of Observation.	Average error.	Range bet. greatest and least.	Coefficient.
	s.	s.	
Greenwich—Edinburgh. Observations at each station, compared final result with truth	2.4	21.8	9.0
Washington—Greenwich. Observations at each station, compared final result with truth.....	2.1	21.6	10.3
Hudson—Greenwich, compared with mean result for each station.....	6.3	17.4	2.9
Brussels—Greenwich, compared with mean result for each station	5.0	17.6	3.5
Places in Turkey with Paris, compared with mean result at each station	6.7	27.8	4.1
Camp Riley, comp'd of mean with tabular places..	14.0	55.0	3.9
Willet's Point, compared with transit observations—			
From American Ephemeris.....	9.3	43.0	4.6
From British Ephemeris.....	14.0	55.0	3.9
Lunars compared with known positions.....	40.7	97.1	2.4

For fixed observatories, with the most perfect instruments, we must therefore expect from the lunar method of moon culminations an absolute error of 2.2s. within a range of 21.6s. as the result of any number of observations. These results correspond in a general way with an investigation made by Professor Pierce. He found the ultimate limit to be ± 0.55 s. when one limb of the moon was observed. He says : "Beyond this it is impossible to go with the utmost refinement. By heaping error upon error, it may crush the influence of each separate determination, but does not diminish the relative height of the whole mass of discrepancy. But this discrepancy between the results for different limbs of the moon often amounts to 10s. in the mean determination of a year." The assumption that the ultimate limit of accuracy is as great as 1s. seems to be a very moderate widening of the limits. I find it to be 2.2s.

For fixed observatories, comparing with the mean result at any station, we must expect a relative error of 6.0s., with a range of 20.9s.

For fixed observatories, using the moon's tabular places, we must expect an error of 12.4s. with a range of 51.0s.

For lunar distances, with the sextant, on land, we must expect an error of 40.7s., with a range of 97.1s.

For single lunar observations at sea these quantities should have a coefficient at least as great as three *additional* units.

It will be noticed that the coefficient is much larger for comparisons with known values than for comparisons with the mean of a given series.

I now take up the subject of chronometers, and make a similar investigation.

The sources of error are:

(a) Variations of rate, arising from the action of magnetism.

The only observations under this head that I can find are those made by Mr. Fisher and by Professor Airy. Mr. Fisher made a very elaborate series of experiments, from which he found that the earth's magnetism changed the daily rate in one case 4.5s., in another 3.2s., and in another 4.1s.

Professor Airy made similar experiments and found similar results, the extreme variation on account of the influence of terrestrial magnetism being 5.8s. He found the following rates for Brockbanks, No. 425:

When the figure—

XII	was north,	rate =	4.64s.
"	" east	" =	8.70
"	" south	" =	9.61
"	" west	" =	5.75

He also found that the action of terrestrial magnetism could be eliminated by placing the chronometer on the top of a compass-box whose needle was perfectly free, provided the elevation was properly adjusted. When this adjustment was made he found for—

XII N.	rate =	9.24s.
" E.	" =	9.41
" S.	" =	9.75
" W.	" =	10.03

This corresponds with my own observations in the case of clocks. I have long noticed that the Cambridge and Boston clocks gain and lose together, due to some kind of a sympathetic action between the two.

(b) When chronometers are swung on the same support it is probable there is a sympathetic action between them similar to the results recently found with the transit of Venus clocks.

(c) Variation on account of change of barometric pressure. This varies between .3s. and .8s. per day for every inch of change in the barometer.

(d) Variation between land and sea rates. The elder Professor Bond made a full investigation of the *average* difference between the land and sea rate, and found them essentially the same. It had been previously assumed that there was an average gain of the latter over the former. But in individual cases there are, without doubt, great changes of the rate, arising mostly from careless handling in transportation to the ship.

Almost every chronometer will change its rate when its circumstances, either of rest or motion, are changed. The Boston standard clock of Messrs. Bond & Son almost invariably has a different rate on Sunday from any other day of the week. So, also, it takes a new rate when the streets are covered to any considerable depth with snow.

The jar of machinery affects the rate; hence the rate of a sailing vessel is more steady than that of a steamer.

(e) Variation of rate at sea, on account of change of temperature.

Mr. Hartnup, of Liverpool, was the first to give the chronometer rate for different temperatures. So far as I can find he is the only one who does so now, and yet failure in this respect occasions errors of enormous magnitude. In one case which Mr. Hartnup instances, if the navigator had relied upon his *average* rate he would have been nearly sixty miles in error. This is a reform in rating chronometers which is imperatively demanded, and it is one easily accomplished.

Let us now attempt to determine the limits within which a chronometer can be depended on to give the longitude at sea. Here we are limited in a great measure to the performances of chronometers on land, inasmuch as at sea we have no means of comparison with a normal standard. The chronometer tests at the Greenwich observatory, however, afford abundant facilities for ascertaining the performance of chronometers under varying conditions of temperature. I select for discussion three series—the first from 1842 to 1852, the second from 1853 to 1862, and the third from 1863 to 1871. The quantities to be determined are the greatest difference between the rates during a given trial, which usually extended over a period of about six months, and the greatest difference of rates between one week and the next following.

The only rule followed in the selection of the chronometers chosen for these tests has been the preference given to makers who entered their chronometers the greatest number of times during the entire interval from 1842 to 1871. The method of proceeding was as follows: Hav-

ing selected for this test, *e. g.*, the chronometers of Frodsham, the quantities sought were obtained from the chronometer tests recorded in the volumes of the Greenwich observations between the years 1842 and 1871. Inasmuch as the value given for each group is a mean value, derived from the number of chronometers entered for trial between the limiting dates, I have added the greatest value for a given series. The addition of these columns is important, since the *liability* to error is as great as the greatest range of error.

The temperature at which the comparisons were made varies from about 35° to about 95° Fahrenheit. The variation of rate under nearly the same temperature will appear from the column "Greatest range in daily rate between one week and the next following."

CHRONOMETER RATES DETERMINED AT THE GREENWICH OBSERVATORY UNDER VARYING CONDITIONS OF TEMPERATURE.

Maker	Frodsham.	Dent.	Reid.	Fletcher.	Lister.	Roskell.	Eiffe.	Kullberg.	Poole.	Loseby.	Cottrell.	Massey.	Appleton.	Mean by weights.
Number of Chronometers.	{ 1842-52 { 1853-62 { 1863-71	9 6 9	10 10 9	7 9 8	7 2 7	6 10 8	4 5 5	7 9 4	.. 3 7	9 10 5	7 1 ..		9	7
Greatest range of daily rate during trial.	{ 1842-52 { 1853-62 { 1863-71	<i>s.</i> 3.37 2.67 2.41	<i>s.</i> 2.74 3.91 1.79	<i>s.</i> 3.20 3.25 2.24	<i>s.</i> 3.74 3.47 3.72	<i>s.</i> 4.40 3.64 2.06	<i>s.</i> 5.56 6.11 2.71	<i>s.</i> 2.43 5.00 3.54	<i>s.</i> .. 3.72 1.85	<i>s.</i> 3.21 3.14 2.20	<i>s.</i> 2.07 3.24 2.20	<i>s.</i> 3.70 3.39 ..	<i>s.</i> 3.39	<i>s.</i> 2.90 3.83 2.41
Greatest range in given series.	{ 1842-52 { 1853-62 { 1863-71	5.56 4.16 4.40	7.74 5.31 2.91	5.90 4.70 2.87	5.59 4.14 3.49	8.73 7.00 2.41	9.11 1260 4.24	3.39 7.50 5.60	.. 5.60 2.63	5.50 6.77 2.77	2.59 3.24 ..	6.40	7.57	3.97 6.10 3.48
Greatest range of daily rate bet. 1 week and the next following.	{ 1842-52 { 1853-62 { 1863-71	1.87 1.79 1.20	1.54 1.93 1.01	2.06 1.73 1.07	1.56 1.34 0.87	2.50 1.79 1.16	3.24 2.70 1.81	1.67 2.29 2.47	.. 1.33 0.83	1.93 1.63 1.13	0.99 2.17 ..	2.14	1.76	1.76 1.88 1.19
Greatest range in given series.	{ 1842-52 { 1853-62 { 1863-71	3.04 2.79 2.34	2.87 3.30 1.90	4.91 3.64 1.60	3.01 1.43 1.53	3.63 3.71 1.97	6.37 6.49 4.11	3.10 5.57 4.87	.. 1.53 1.36	3.53 4.60 1.44	1.60 2.17 ..	4.51	5.00	3.00 3.52 2.35

In order to exhibit still further the variations due to a change of temperature I have selected from the Greenwich reports for the years 1867-68-69-70-71 five chronometers showing the best performance, and five showing the poorest performance. The observations cover a period of four weeks, during which time the chronometers were subjected to a high temperature (about 95°), and during the period of four weeks immediately preceding, under ordinary temperature.

The headings explain the operations performed.

Year.	Class.	Maker.	Mean daily rate under ordinary temperature.	Mean daily rate under high temperature.	Var'n bet. ordinary and high temp.	Range of daily rate under ordinary temp.	Range of daily rate un. high t.
			<i>s.</i>	<i>s.</i>	<i>s.</i>	<i>s.</i>	<i>s.</i>
1867.	Best....	Sewill....	-2.21	-0.55	.66	.23	.77
		Gowland ..	.85	-1.21	.36	.51	.31
		Birchall..	-1.13	-1.11	.02	.11	.41
		Dent.....	+1.51	+2.12	.61	.34	.90
		Hennesy..	.59	- .43	.16	.99	.41
	Poorest	Lister....	+2.37	+0.57	2.94	0.39	0.43
		Shepherd +	1.29	+1.75	0.46	.73	.17
		Poole....	-0.25	+3.83	4.08	.80	1.49
		Crisp.....	+7.96	+10.34	2.38	.79	1.63
		Johansen +	0.12	+0.43	0.31	1.30	2.04
1868.	Best ...	Birchall..	.50	- .34	.16	0.14	.21
		Fletcher..	-1.66	-1.71	.05	.23	.63
		Davison..	.62	-1.01	.39	.34	.74
		Gowland ..	-1.61	-1.24	.37	.36	.56
		Young ..	.73	-1.01	.28	.19	.23
	Poorest	Sewill....	+1.13	+0.86	.27	0.19	3.29
		"	+ .05	+1.91	1.86	.86	.36
		Birch	+1.16	+1.09	2.25	1.21	.76
		Dell.....	+1.30	+3.21	1.91	1.64	.56
		Glover....	.63	+ .84	1.47	.79	2.18
1869.	Best....	Fletcher..	-0.52	-0.62	.10	.30	.20
		Whiffen..	.17	- .58	.41	.79	.04
		Dent.....	.29	- .85	.56	.33	.04
		Kullberg ..	.12	+ .34	.46	.43	.30
		"	+ .20	+ .24	.04	.36	.46
	Poorest	Carter....	+0.39	- .51	.90	.67	.61
		Glover....	-7.67	-5.02	2.65	.80	1.84
		Fletcher..	-2.64	-3.47	.83	.36	.36
		Cairns....	.51	-2.51	2.00	.51	1.07
		Shepherd ..	-3.28	+0.03	3.31	.44	.61
1870.	Best....	Dent.....	.13	- .54	.41	.34	.51
		Chittend. +	.32	- .09	.41	.31	.46
		Reid	.76	- .34	.42	.54	.53
		Lowry ...	.91	-1.00	.09	.20	.60
		Kingston +	.28	- .48	.76	.86	.21
	Poorest	Reid	-1.34	- .50	.84	.91	.91
		Webb.....	.05	-2.52	2.47	.33	.94
		Whiffin..	-2.06	+ .51	2.57	.29	1.81
		Eiffe.....	-2.53	- .98	1.55	.71	1.71
		Gowland..	.46	-3.92	3.46	1.83	3.01
1871.	Best....	Frodsham-	.93	-1.23	.30	.83	.34
		Weichert ..	.54	- .68	.14	.69	.51
		Kingston-	.33	- .73	.40	.33	.41
		Dell.....	-1.12	-1.19	.07	.54	.31
		Gowland +	.39	+ .88	.49	.43	.56
	Poorest	Gowland ..	-2.43	-1.18	1.25	.19	1.14
		Hennesy..	.08	-3.09	3.11	.49	.79
		McGregor-	3.25	+ .84	4.09	.70	.36
		Shepherd-	.28	-3.75	3.47	.33	.74
		Reid.....	-1.88	+2.59	4.47	.30	.86

Summary.

Year.	Mean diff. of daily rate between high and ordinary temperatures.	Greatest variation of daily rate.	Range of daily rate under ordinary temperature in 1 mo.	Range between different chronometers.	Range of daily rate under high temperature in 1 mo.	Range between different chronometers.
Best.						
	s.	s.	s.	s.	s.	s.
1867	.36	.66	.44	.88	.56	.51
1868	.25	.39	.25	.22	.47	.53
1869	.32	.56	.44	.49	.21	.42
1870	.39	.76	.45	.55	.46	.39
1871	.28	.49	.56	.50	.43	.25
	—	—	—	—	—	—
Means	.32	.57	.43	.53	.43	.42

Poorest.						
1867	2.03	4.08	.80	.91	1.15	1.87
1868	1.55	2.25	.94	1.45	1.43	2.93
1869	1.94	3.39	.53	.54	.90	1.48
1870	2.18	3.46	.81	1.54	1.70	2.01
1871	3.28	4.47	.40	.51	.78	.78
	—	—	—	—	—	—
Means	2.20	3.51	.70	0.97	1.19	1.81

From these tables I draw the following conclusions :

(a) There was a very marked and steady improvement in the construction of chronometers between 1842 and 1871. The greatest range of daily rate during the entire trial fell from 3.40s. in 1842-52 to 2.41s. in 1863-71. The greatest range of the daily rate between one week and the next following was reduced from 1.91s. in 1842-52 to 1.19s. in 1863-71. These were the palmy days of chronometer construction. I have made only a partial discussion of the trials since 1871, but the evidence is pretty conclusive that the averages are larger than for the period 1863-71.

(b) Under the most favorable conditions for the excellent performance of chronometers, the average change of the rate between one week and the next following exceeds 1.2s. Of course, with the newly acquired rate the accumulation of error will be proportional to the time until another change takes place.

(c) The *average* liability to error in the daily rate between one week and the next following is not far from 3 seconds.

(d) The ratio-value between the average of first-class chronometers and the average of poor ones is nearly as 1 to 7.

ON THE MAGNITUDE OF THE ERRORS OF CHRONOMETERS EMPLOYED IN SHORT AND REGULAR SEA VOYAGES.

It will be allowed on every hand that a discussion of the actual errors of the chronometers used on the Cunard line of steamships will exhibit results considerably better than the general average. The well-known skill of the navigators in the service of this company, the perfection of every appliance, the general sameness of the conditions of each voyage, the number of chronometers employed, and the accurate rating of each chronometer at the beginning and end of each voyage by comparison with the time from fixed observatories, all conspire to give the most favorable results which can be expected from the performance of chronometers in the present state of chronometer construction. With the assistance of Mr. Aug. McConnell, I have made a discussion of the chronometer errors of the Cunard steamers sailing between Liverpool and Boston during the years 1871-1872 and 1873.

Chronometer Rates on Cunard Steamers.

Steamer.	Date of arrival at Boston.	Length of voyage.	Total error at end of Voyage.			Mean error at end of voyage.	Range of error between Nos. 1, 2 and 3.	Mean error of daily rate.
			Chron. No. 1.	Chron. No. 2.	Chron. No. 3.			
			s.	s.	s.	s.	s.	s.
PALMYRA.	1871, Nov.	20. 14 days.	+ 9.8	- 2.8	- 5.6	0.5	15.4	0.03
	1872, Feb.	29. 17	+ 1.7	+ 1.7	-20.4	5.7	22.1	0.33
	" April	8. 14	- 9.8	- 5.6	- 5.6	7.0	4.2	0.50
	" May	17. 16	- 9.6	-25.6	-62.4	32.5	52.8	2.03
	" June	9. 14	+ 5.6	-25.8	-21.0	13.1	29.4	0.93
	" July	24. 14	- 2.8	- 4.2	+ 5.6	0.5	9.8	0.03
	1873, March	20. 17	+ 0.0	+ 5.1		2.5	5.1	0.15
OLYMPUS.	1872, April	18. 15	- 3.0	- 1.5	+ 1.5	1.0	4.5	0.07
	" May	20. 12	+12.0	+ 6.0	+18.0	12.0	12.0	1.00
	" June	25. 13	- 5.2	- 1.3	+13.0	2.2	18.2	0.17
	" July	31. 14	+14.0	+ 0.0	+ 8.4	7.5	14.0	0.53
	" Sept.	3. 13	+ 5.2	+ 1.3	+ 1.3	2.6	3.9	0.20
	" Nov.	13. 16	+ 4.8	+ 6.4	- 3.2	2.7	9.6	0.17
	" Dec.	18. 16	+ 4.8	+ 8.0		6.4	3.2	0.40
	1873, Feb.	3. 21	+12.6	+ 2.1	-12.6	0.7	25.2	0.03
	" March	13. 17	+10.2	+15.3	+ 5.1	10.2	10.2	0.60
SAMARIA.	1871, Dec.	5. 15	+ 1.5	+27.0		12.7	28.5	0.85
	1872, Jan.	15. 21	- 2.1	+46.2	+ 0.0	14.7	48.3	0.70
	" Feb.	20. 15	+ 9.0	+ 24.0	+ 3.0	12.0	21.0	0.80
	" March	27. 16	+ 9.6	+14.4	+ 0.0	8.0	14.4	0.50
	" May	6. 13	+ 3.9	+13.0	+ 9.1	8.7	9.1	0.67
	" June	11. 13	- 1.3	+ 2.6	+ 1.3	0.9	3.9	0.07
	" July	16. 13	- 6.5	+ 0.0	- 1.3	2.6	6.5	0.20
	" Aug.	19. 12	- 1.2	+ 0.0	+ 3.6	0.8	4.8	0.07
	" Sept.	24. 13	- 3.9	- 1.3	- 1.3	2.2	2.6	0.17
	" Oct.	29. 13	- 2.6	+11.7	-19.5	3.5	31.2	0.27
	" Dec.	13. 13	+10.8	+ 5.4	+14.4	10.2	9.0	0.57
	1873, Jan.	17. 18	+10.8	+ 1.8	- 1.8	3.6	12.6	0.20
	" March	3. 14	+18.2	- 1.4	+ 8.4	8.4	19.6	0.60

Chronometer Rates on Cunard Steamers—Continued.

Steamer.	Date of arrival at Boston.	Length of voyage.	Total error at end of Voyage.			Mean error at end of voyage.	Range of error between Nos. 1, 2 and 3.	Mean error of daily rate.
			Chron. No. 1.	Chron. No. 2.	Chron. No. 3.			
			s.	s.	s.	s.	s.	s.
PARTHIA.	1871. Nov.	27. 14 days.	+ 5.6	+ 4.2	+1.4	3.7	4.2	0.26
	1872. Feb.	5. 14	+ 9.8	+ 9.8	+ 9.8	9.8	0.0	0.70
	" March	29. 11	+ 3.3	- 3.3	+ 8.8	2.7	12.1	0.26
TRIPOLI.	1871. Dec.	19. 15	+ 6.0	- 1.5	- 1.5	1.0	7.5	0.07
	1872. Feb.	2. 18	- 1.8	+ 5.4	- 5.4	0.6	10.8	0.03
	" March	14. 16	+ 3.2	+22.4	+ 9.6	11.7	19.2	0.73
HECLA.	" April	24. 14	+ 2.8	- 9.8	- 8.4	5.1	12.6	0.36
	1872. Jan.	17. 16	+ 0.0	- 1.6	- 1.6	1.1	1.6	0.07
	" April	29. 12	-12.0	+ 3.6	+ 2.4	2.0	15.6	0.17
SIBERIA.	" June	4. 13	- 5.2	+ 2.6	-14.3	5.6	16.9	0.43
	" July	10. 14	- 1.4	+ 2.8	+18.2	6.5	19.6	0.46
	" Aug.	13. 13	+ 2.6	+16.9	+ 1.3	6.9	15.6	0.53
HECLA.	" Sept.	17. 13	-10.4	+11.7	+10.4	3.9	22.1	0.30
	" Oct.	23. 14	- 7.0	- 1.4	- 2.8	3.7	5.6	0.26
	" Dec.	5. 17	-10.2		-13.6	11.9	3.4	0.70
SIBERIA.	1873. Jan.	13. 21	- 4.2	+25.2	-14.7	2.1	39.9	0.10
	" Feb.	17. 14	+ 4.2	- 7.0	+22.4	6.5	29.4	0.46
	" April	1. 15	-16.5	- 7.5	- 3.0	9.0	13.5	0.60
SIBERIA.	1871. Dec.	14. 15	+31.5	+ 4.5	+24.0	20.0	27.0	1.33
	1872. Jan.	26. 18	+ 3.6	+ 5.4	+16.2	8.4	12.6	0.47
	" March	5. 15	- 9.0	+ 1.5	+ 6.0	0.5	15.0	0.03
SIBERIA.	" April	23. 13	- 9.1	- 3.9	+ 0.0	4.3	9.1	0.33
	" May	28. 13	+ 2.6	+ 0.0	+ 7.8	3.5	7.8	0.27
	" July	3. 14	- 4.2	+12.6	+ 4.2	4.2	16.8	0.30
SIBERIA.	" Aug.	5. 12	+ 6.0	+ 4.8	+ 3.6	4.8	2.4	0.40
	" Sept.	10. 13	- 1.3	+ 7.8	+ 3.9	3.5	9.1	0.27
	" Oct.	16. 14	+ 5.6	+11.2	+ 9.8	8.9	5.6	0.63
SIBERIA.	" Nov.	20. 14	- 1.4	+ 9.8	-36.4	9.3	46.2	0.63
	" Dec.	31. 22	+ 2.2		+ 4.4	3.3	2.2	0.15
	1873. Feb.	7. 18	+ 9.0	+ 0.6	- 5.4	1.2	14.4	0.07
SIBERIA.	" April	14. 12	+ 1.2	- 9.6	- 3.6	4.0	10.8	0.33
BATAVIA.	1872. Jan.	3. 16	+ 4.8	+ 3.2	+ 3.2	3.7	1.6	0.23
	" Feb.	15. 17	+18.7	+ 3.4	+ 0.0	7.4	18.7	0.43
	" March	20. 16	- 1.6	- 9.6	- 6.4	5.9	8.0	0.37
BATAVIA.	" Nov.	25. 14	- 1.4	+22.4	+ 8.4	9.8	23.8	0.70
	1873. Jan.	20. 14	- 7.0	+ 5.6	+ 8.4	2.3	15.4	0.16
	" Feb.	25. 15	- 4.5	+ 7.5	+ 4.5	2.5	12.0	0.17
BATAVIA.	" April	7. 14	- 2.8	+ 2.8	+ 1.4	0.5	5.6	0.03
MALTA.	1872. Aug.	29. 15	-12.0	- 4.5	-63.0	26.5	58.5	1.77
	" Sept.	30. 12	- 7.2	-34.8	-14.4	18.8	27.6	1.57
	" Nov.	7. 15	+ 3.0	+ 0.0	+ 4.5	2.5	4.5	0.17
MALTA.	1873. Jan.	4. 19	-15.7	+20.9	+11.	5.5	33.6	0.29
	" Feb.	12. 16	+ 0.0	-25.6	+ 0.0	8.5	25.6	0.65
	" March	24. 14	- 7.0	-23.8	- 2.8	11.2	21.0	0.80

Average Results.

Steamer.	No. of voyages.	Av. length of voyage.	Av. Error of daily rate.	Av. error at end of voyage.	Range between greatest and least value of series.	Range between three chronometers.	Greatest value of series.
			s.	s.	s.	s.	s.
Palmyra,	7	14.7 days.	±0.57	8.83	32.0	19.83	52.8
Olympus,	9	15.2	0.85	5.03	11.3	11.20	25.2
Samaria,	13	14.9	0.44	6.77	13.9	16.27	48.3
Hecla,	11	14.7	0.37	5.29	10.8	16.65	39.9
Parthia,	3	13.0	0.41	5.47	6.9	5.43	12.1
Tripoli,	4	15.8	0.30	4.60	11.1	12.52	19.2
Siberia,	13	14.9	0.40	5.84	19.5	13.77	46.2
Batavia,	7	15.2	0.30	4.59	9.3	12.16	23.8
Malta,	6	15.2	0.86	12.16	24.0	28.97	58.5
Means,	8	15.4	0.44	6.52	15.42	15.20	36.22

Chronometer Rates on Sailing Vessels.

The chronometers in this discussion were selected without regard to rates, except when the variation exceeded 2s., and the chronometers needed repairs at the end of the voyage. The materials for this part of the discussion were furnished by Mr. McConnell.

Maker.	No. of chron.	Length of voyage	Total error at end of voyage. s.	Maker.	No. of chron.	Length of voyage.	Total error at end of voyage. s.
Bond & Sons.	219	182 days.	18.2	Bliss & Creighton.	818	122 days.	48.8
	239	30	0.0		1307	81	16.2
	253	84	33.6		1823	99	128.7
	163	100	30.0		878	64	64.0
	173	71	0.0		782	144	86.4
	278	44	4.4		1131	80	160.0
	232	30	15.0		1888	3	21.0
	193	178	17.8		2004	77	154.0
	275	45	4.5		1850	48	24.0
	192	74	37.0		815	82	32.8
Hutton.	265	134	12.4	Negus.	1091	51	30.6
	195	93	39.2		709	93	46.5
	492	78	7.8		1151	31	15.5
	366	147	44.1		1000	107	21.4
	218	81	5.4		1178	88	88.0
	351	141	42.3		812	181	72.4
	382	21	14.7		1154	95	28.5
	493	50	25.0		848	44	11.4
	289	20	12.0		1048	55	16.5
	160	40	12.0		829	113	33.9

Maker.	No. of chron.	Length of voyage.	Total er- ror at end of voyage. s.	Maker.	No. of chron.	Length of voyage.	Total er- ror at end of voyage. s.
Garraud.	5175	102 days.	14.4	Frodsham.	2267	149 days.	74.5
	2182	173	51.9		1863	47	23.5
	2552	77	61.6		2470	96	124.8
	776	0	6.7		1808	109	98.1
	2185	149	44.7		2528	135	202.1
	321	221	154.7		1945	138	41.4
	5210	56	16.8		2565	193	38.6
	2614	34	17.0		2427	56	39.2
	5117	219	87.6		2502	263	0.0
	1965	177	141.6		2690	62	37.2
Poole.	3336	14	5.6	Tobias.	1302	7	1.4
	2400	193	146.3		1472	23	13.8
	1920		37.8		1200	67	13.4
	1712	258	77.4		1446	52	46.8
	1299	21	31.5		1386	215	64.5
	1674	1	50.5		1673	34	6.8
	2477	49	4.9		1296	14	8.4
	3849	14	4.2		1509	229	0.0
	1628	190	19.0		1003	30	36.0
	3803	14	15.4		1216	104	104.0
Adams.	3895	294	88.2	10 other makers.	6582	76	0.0
	4225	112	156.8		2820	26	13.0
	2777	80	8.0		1965	19	5.7
	4118	75	7.5		619	78	7.8
	4328	138	165.6		654	101	50.5
	723	19	3.8		259	39	66.3
	534	84	42.0		5224	115	207.0
	766	21	4.2		927	63	37.8
	656	3	.9		902	83	24.9
	1102	2	1.0		963	113	22.6

Collecting results, we have:

Maker.	Av. length of voyage.	Av. error of daily rate. s.	Av. error at end of voyage. s.
Bond & Sons,	84 days.	0.19	16.1
Bliss & Creighton,	80	0.92	73.7
Hutton,	75	0.29	21.6
Negus,	86	0.42	36.5
Berraud,	127	0.47	59.7
Frodsham,	125	0.54	67.9
Poole,	98	0.40	39.3
Tobias,	78	0.38	29.5
Adams,	83	0.58	47.8
10 other makers,	71	0.61	43.6
Means,	91	0.48	43.6

The longitude expeditions undertaken by Professor Wm. C. Bond in 1849-50, and by Professor George P. Bond in 1855, for the accu-

ate determination of the longitude between Cambridge and Greenwich, furnish the most reliable data available for a correct estimate of the degree of accuracy to be expected from the average performance of a chronometer during a short sea voyage. On these expeditions the rates of all the chronometers employed were carefully determined at each station.

In this discussion only those chronometers are selected which made three or more voyages. The longitude given by each chronometer is compared with $\lambda = 4\text{h. } 44\text{m. } 30.9\text{s.}$ The numbers given are current numbers, and not the numbers given to the chronometers by the makers. The values given under each number are the separate corrections to the assumed longitude given by the same chronometer on different voyages. The exceptional value of these determinations justifies the publication of the details.

EXPEDITION OF 1849-50.

Nos.	1	2	3	4	5	6	7	8	9	10	11
Corrections by each Chronometer to assumed value of λ .	$s.$ +0.1 -13.6 +7.6	$s.$ +5.7 +0.6 +0.2	$s.$ +3.0 -7.1 -1.1	$s.$ +3.4 +1.2 +0.1	$s.$ +23.2 +2.9 +0.0 -16.5 -6.3 +6.8	$s.$ +3.2 +1.8 +1.5	$s.$ +3.6 -3.3 -4.4	$s.$ +3.8 -1.2 +3.0	$s.$ -0.1 -1.7 +1.7	$s.$ +8.2 +2.2 -7.6	$s.$ +0.1 +1.4 -1.9
Means	+1.97	+2.17	-1.73	+1.57	+2.18	+3.17	-1.37	+1.87	-0.03	+0.93	+1.13
	12	13	14	15	16	17	18	19	20	21	22
	$s.$ -7.4 -15.3 -14.8 +10.6	$s.$ -5.0 +1.9 +1.5 +12.5	$s.$ +6.5 -1.5 -5.3	$s.$ -10.9 -4.4 -14.5	$s.$ +3.4 +3.8 +1.9	$s.$ -0.6 -1.9 +10.0 -7.8 +8.9	$s.$ -1.8 -1.9 +6.0 +3.8	$s.$ -0.3 +10.5 +11.7 -6.1 -10.9 -12.8	$s.$ +11.0 -2.8 +5.5 -0.6 -0.9	$s.$ +12.9 +15.5 -10.1 +19.5 +0.9 +2.2	$s.$ +4.3 +13.0 +2.8 -18.4 -20.3 +4.8
Means	-6.72	+2.73	-0.16	[-9.93]	+3.03	+1.72	+0.78	-1.32	+2.68	+2.52	-2.30
	23	24	25	26	27	28	29	30	31	32	33
	$s.$ +3.1 +13.8 -8.3 -6.1	$s.$ +2.9 +5.9 -1.4 +9.0 -12.5 -9.1 +3.1	$s.$ +2.9 -3.3 -11.7 +10.7 -4.2 +1.7 +9.3	$s.$ +7.5 +16.0 -10.4 -39.0 -11.7 -1.4 +1.2	$s.$ +6.1 -8.5 +20.1 +4.0 -34.5 -5.0 -9.0	$s.$ +4.3 +14.6 -3.9 +8.8 -8.5 -1.0 +5.2	$s.$ +0.1 +7.1 +5.8 +16.5 -6.7 -5.0 -5.3	$s.$ +0.9 +10.1 +5.1 +5.9 +0.1 -6.5 -5.1	$s.$ +4.0 +15.5 -3.6 +8.3 -13.5 +0.1 -15.0	$s.$ -2.3 +12.5 -11.4 +10.9 +0.8 +1.0 +5.4	$s.$ -0.2 +15.2 -14.0
Means	+1.88	-0.30	+0.77	-5.00	-3.83	+2.78	+1.78	+1.50	-0.60	+2.41	+0.33

EXPEDITION OF 1855.

Nos.	1	2	3	4	5	6	7	8	9	10	11	12
Correcti'ns by each Chronome- ter to assumed value of λ .	s_1 +2.8 +0.9 +5.7 +1.9 +0.9	s_2 +1.8 -4.2 +3.2 -1.9 —	s_3 -15.8 -2.4 -5.9 +0.3 -4.5 -1.1	s_4 -7.0 +1.1 -5.8 +0.7 -0.7 +0.3	s_5 +3.5 -0.2 -0.1 -0.7 -6.4 -5.9	s_6 -15.8 -5.1 -1.9 +2.4 -0.7 -0.6	s_7 -12.5 -2.0 -4.8 +0.8 -2.5 -0.5	s_8 -2.9 +0.4 -0.9 -1.1 +0.2 -1.0	s_9 -6.1 -2.7 -1.4 -1.8 -3.6 -0.8	s_{10} (-74.0) +1.4 -5.1 -8.9 -3.2 +2.8	s_{11} (-46.2) +5.7 -8.5 -7.7 -4.3 +2.7	s_{12} (-39.2) -3.8 -0.6 -5.3 -2.4 +1.7
Means.	+3.04	-0.27	-4.90	-1.90	-1.63	-1.02	-3.58	-0.88	-2.73	-2.60	-2.42	-2.08
	13	14	15	16	17	18	19	20	21	22	23	24
	s_1 -6.2 -0.2 -4.4 +6.0 -4.2 +2.6	s_2 +1.8 -4.9 +2.5 -1.2 +2.2 -1.4	s_3 -2.6 -4.8 +0.7 -0.5 +2.3 -6.3	s_4 -0.6 -1.4 +0.3 -0.3 +1.8 +1.7	s_5 -4.1 +2.7 +0.5 -8.2 -2.8 -1.7	s_6 -11.2 +1.9 -11.2 +12.0 -4.8 +3.9	s_7 -2.7 -11.5 +5.8 -1.6 -2.4 +2.4	s_8 +8.7 -0.8 -1.5 +8.5 -1.2 +1.2	s_9 +0.0 -0.5 +5.4 +3.4 -5.1 -11.1	s_{10} -1.0 -5.3 -4.0 +0.0 +2.4 -3.4	s_{11} +1.1 -0.8 +10.8 -4.1 +0.1 -0.3	s_{12} -1.3 -5.5 -2.4 -2.3 -3.3 -1.7
Means.	-1.07	+1.47	-1.87	+0.35	-2.27	-1.57	-1.67	+2.48	-1.15	-1.88	+1.13	-2.75
	25	26	27	28	29	30	31	32	33	34	35	36
	s_1 +0.5 -2.6 +1.5 -1.0 +1.5 -0.7	s_2 +3.4 -0.4 -0.2 -4.7 -1.1 —	s_3 +0.0 -0.9 -0.2 -0.8 -0.7 -1.2	s_4 +0.3 -6.4 -2.8 -4.2 +0.0 -4.6	s_5 -7.9 -3.7 -6.3 -0.3 -1.5 -0.9	s_6 -3.1 -9.8 +0.1 +4.7 +0.5 +5.2	s_7 -5.8 -0.1 -4.8 +7.6 -6.5 +11.9	s_8 -3.1 -0.2 -2.5 +0.5 -1.8 -2.1	s_9 -16.4 +4.8 -10.6 -0.4 +0.2 +1.1	s_{10} +7.0 -1.9 +4.7 -10.1 -5.2 +4.3	s_{11} -0.2 +1.9 -4.1 -0.6 +3.5 -0.8	s_{12} +7.6 -5.9 +4.7 +5.8 -0.5 +1.7
Means.	-0.13	-0.60	-0.63	-2.95	-3.28	-0.40	+0.38	-1.53	-3.55	-0.20	-0.05	+2.23
	37	38	39	40	41	42	43	44	45	46	47	48
	s_1 +5.0 +0.1 -3.8 -1.2 -2.7	s_2 -1.4 +2.2 -4.1 -0.5 -2.5	s_3 -1.5 -3.4 +0.5 -3.9 +3.0	s_4 -3.0 -1.5 +0.9 -0.2 —	s_5 -4.9 -1.3 -0.2 —	s_6 -7.2 -0.2 -0.4 —	s_7 -1.7 -2.2 -2.5 —	s_8 +6.2 -0.4 -5.7 —	s_9 -1.4 -0.6 -1.2 —	s_{10} -7.1 +9.8 -3.7 -6.2 -1.6 +2.1	s_{11} +4.6 -3.1 -2.3 +6.4 -4.0 +1.0	s_{12} -3.0 -1.2 -9.0 -3.3 +2.8 -3.1
Means.	-0.52	-1.26	-1.06	-1.20	-2.13	-2.60	-2.13	+0.03	-1.07	-1.12	+0.43	-2.80

Combining the mean corrections given by each chronometer on different voyages, without regard to weights, we have the following results:

	Mean Corr.
Expedition of 1849-50	+ .63s.
Expedition of 1855	-1.18
Final Correction	-0.27

In order to form an estimate of the *liability* to error in any chronometer, we must compare the greatest and the least corrections given by the same chronometer on different voyages. The *average* range for 1849-50 is 19.5s., and for 1855 it is 10.8s. The extreme range is shown in the following table:

Limits	s	s	s	s	s	s	s	s	s	s	s
55	50	45	40	35	30	25	20	15	10	5	0
..	to	to	to	to	to	to	to	to	to	to	to
..	55	50	45	40	35	30	25	20	15	10	5

Expedition	Number of Cases.										
1849-50.	0	2	0	1	0	4	1	8	4	4	5
1855.	0	1	0	1	1	0	0	2	6	8	19

The values given on pages 370 and 371 represent absolute corrections from which the error of rate has been eliminated. It is not possible, therefore, to determine the average error of the daily rate directly from these series. It can be obtained, however, somewhat indirectly from the table given on page 368. It appears from this table that the average range between the greatest and the least values of the average errors at the end of a voyage of 14.8 days is 15.42s.; and that the coefficient by which this average error must be multiplied in order to produce the latter value is 2.37. The mean of the average range for the expedition for 1849-50 and that for 1855 is 15.2s., from which we may with tolerable safety assume the average error at the end of a voyage of 15 days to be not far from 6.4s., giving for the average daily error of rate the value ± 0.43 s., which is almost identical with the values already found on pages 370 and 371.

I add, without details, the results of two other longitude expeditions:

	No. of Chronometers.	Length of Voyage. Days.	Average Error at End of Voyage. s.	Range between greatest and least Value. s.	Coeff.
Cambridge, New York and Chagres	5	12	7.6	26.4	3.5
Eclipse Expedition to Labrador	5	20	3.7	13.1	3.5

Here we have from a limited number of observations the value ± 0.41 s. for the average error of the daily rate.

It must be remembered that the results so far obtained are *average results obtained from a large number of observations made under the most favorable conditions*. They indicate that a chronometer of average quality is at any time liable to change its daily rate by an amount as great as 0.5s. That this value is probably too small will appear from the following table given by Mr. Hartnup, who made the investigation in 1863. The quantities given represent the error at the end of a voyage of the given duration.

Length of Voyage. No. Mos.	Average error from 1700 Chro.	Best 10 in 100.	2d best 10 in 100.	3d best 10 in 100.	4th best 10 in 100.	5th best 10 in 100.	6th best 10 in 100.	7th best 10 in 100.	8th best 10 in 100.	9th best 10 in 100.	Worst 10 in 100.
	Miles.	Miles.	Miles.	Miles.	Miles.	Miles.	Miles.	Miles.	Miles.	Miles.	Miles.
1	6	0	1	1	2	3	4	5	7	9	25
2	14	0	2	4	5	7	9	11	15	24	62
3	23	1	3	6	9	12	15	18	25	41	101
4	33	1	4	8	13	17	22	28	36	61	143
5	44	1	5	10	17	22	29	39	49	84	187
6	56	2	6	13	21	28	37	50	64	108	233
7	69	2	8	16	25	34	46	62	80	134	280
8	82	3	10	19	30	41	55	74	98	159	328
9	95	3	12	22	35	48	65	86	117	184	376
10	108	4	14	26	40	56	75	98	137	208	425
11	122	4	16	30	46	64	86	111	157	233	474
12	136	5	18	34	52	72	97	124	178	258	524

An error of 524 miles in a ship's position may seem to be rather too large an estimate, but Lord Anson, in a voyage around Cape Horn, did better than this. One of his ships was 500 miles out of her reckoning, and one actually made land on the wrong side of the continent, the error of position being over 600 miles.

In 1871 Mr. Hartnup made an additional investigation, obtaining the following results:

	Extreme Difference of mean daily rate between any 2 weeks	Variation of daily rate due to change of temperature.	Mean of extreme difference between any 2 days of each week.
	s.	s.	s.
Mean from 297 chronometers	2.19	1.73	0.98
Mean from 40 poorest	6.28	4.55	2.80
Mean excluding 40 poorest	1.56	1.29	0.70

In order to show how largely chronometer errors may be diminished when the rate is given for different temperatures in the form adopted

by Mr. Hartnup, I add the following table, given by him in this connection, derived from the actual performance of a given chronometer:

Mean Daily Rate.

Date.	Temp.=55°.	Temp.=70°.	Temp.=85°.
April, 1867	— 0.4 ^s	+ 1.4 ^s	+ 2.3 ^s
May, 1868	— 0.6	+ 1.4	+ 2.2
April, 1869	— 1.4	+ 1.7	+ 2.7
November, 1869	— 0.2	+ 1.9	+ 2.8
February, 1871	— 0.2	+ 1.5	+ 2.3

From the first column of Mr. Hartnup's first table I deduce the following values for the error of the daily rate from the mean of 1700 chronometers, assuming the months to be calendar months:

No. months.	Average error of daily rate.	No. months.	Average error of daily rate.
1	± 0.80	7	1.31
2	0.93	8	1.37
3	1.02	9	1.41
4	1.10	10	1.44
5	1.17	11	1.48
6	1.24	12	1.51

I am not sure that I have given the correct interpretation to Mr. Hartnup's values, since it is difficult to understand why the error should increase with the time.

From Mr. Hartnup's second table we have the value $\pm 0.98s$. Adopting the mean between the latter value and $\pm 0.46s$., the value derived from this discussion, we have finally $\pm 0.72s$. as the average daily error of the rate of an average chronometer.

The average coefficient of safety for chronometer errors only derived from this discussion is 3.2. *If, therefore, the navigator has a chronometer of average excellence, he must at the end of twenty days expect from it an average error of 3.6 miles, and he must look out for an error of $3.6m. \times 3.2$, or 11.5 miles.*

It must be borne in mind that these results are independent of the errors of observation with the sextant, which are still to be added.

In estimating the limits within which it is possible to locate the position of a ship at sea by astronomical observations, it is necessary to take into account *all* the errors to which such observations are liable. I shall consider only the method usually employed, viz. the measurement of the altitude of the sun with a sextant at a given time

before it comes to the meridian for *longitude*, and the measurement of its altitude at culmination for *latitude*.

First of all we must estimate the magnitude of the errors to which sextant observations are liable.

They are as follows:

(a) Instrumental errors, such as excentricity, errors of graduation, index error, &c. In a first-class sextant errors of this class often exceed one minute of arc.

(b) Error in noting the time. No observer at sea pretends to note the time closer than 1s. In fact it is impossible for him to do so. If we assume the low limit of 1s. and multiply by the coefficient 3.5 we find an error from this source amounting to nearly one mile.

(c) Error arising from an imperfect sea-horizon. I am convinced that errors from this cause may amount to several miles. In a series of observations made to test the value of Lieut. Beecher's artificial horizon, the range of error was six miles. Nor does the use of an artificial horizon mend the matter, for there is always an undetermined constant between the two methods which ranges from two to eight miles.

(d) Errors arising from the use of approximate data. It is almost universally the practice to take data to the nearest minute of arc, *e. g.* it is the practice to take the sun's diameter as 30'. Many navigators lump all corrections together and call the sum 12'. The error from this source *may* amount to no less than five miles. Another source of error is the failure to use the value of the refraction corresponding with the actual condition of the atmosphere indicated by the thermometer and barometer at the time of observation. The only observations which I can find bearing upon this point were made by Commander Bayfield at Quebec in 1832, the thermometer reading 11° Fahrenheit. He found that the error arising from the use of the mean instead of the actual refractions amounted to three miles.

(e) Errors affecting the longitude which depend upon the latitude of the ship and upon the declination and the observed altitude of the sun. The errors arising from this source may be much larger than is generally suspected, and they are the more important because, for the most part, they escape the attention of the navigator.

Let,

A = the observed altitude of the sun.

δ = the declination of the sun.

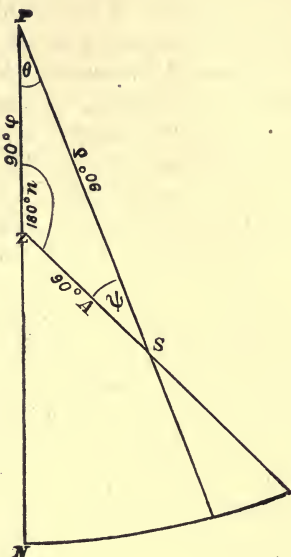
φ = the latitude of the place of observation.

θ = the observed hour angle.

γ = the observed azimuth.

ψ = the angle at the sun.

From the triangle



We obtain

$$\sin. \frac{1}{2} \theta = \sqrt{\frac{\sin. (s - (90^\circ - \delta)) \sin. (s - (90^\circ - \varphi))}{\cos. \delta \cos. \varphi}}$$

where

$$s = \frac{1}{2} [(90^\circ - \delta) + (90^\circ - A) + (90^\circ - \varphi)]$$

$$\sin. \psi = \frac{\sin. \theta \cos. \varphi}{\cos. A} = \frac{\cos. \varphi \sin. \gamma}{\cos. \delta}$$

$$\sin. \gamma = \frac{\sin. \theta \cos. \delta}{\cos. A}.$$

In order to compute the values of the various errors which may affect θ , we differentiate the general equation :

$$\sin. A = \sin. \varphi \sin. \delta + \cos. \varphi \cos. \delta \cos. \theta.$$

From which

$$\begin{aligned} \cos. A dA &= (\sin. \varphi \cos. \delta - \cos. \varphi \sin. \delta \cos. \theta) d\delta \\ &+ (\sin. \delta \cos. \varphi - \cos. \delta \sin. \varphi \cos. \theta) d\varphi \\ &- \cos. \varphi \cos. \delta \sin. \theta d\theta. \end{aligned}$$

But

$$\begin{aligned} \cos. \delta \sin. \varphi - \sin. \delta \cos. \varphi \cos. \theta &= \cos. A \cos. \psi \\ \sin. \delta \cos. \varphi - \cos. \delta \cos. \theta \sin. \varphi &= -\cos. A \cos. \eta \\ \cos. \varphi \sin. \theta &= \cos. A \sin. \psi. \end{aligned}$$

By substitution we easily find :

$$d\theta = -\frac{dA}{\cos. \varphi \sin. \eta} + \frac{d\delta}{\cos. \delta \text{ tang. } \psi} - \frac{d\varphi}{\cos. \varphi \text{ tang. } \eta}$$

In the proceedings of the American Association for 1881, p. 154, Mr. S. C. Chandler has given the following convenient form to this equation :

Dividing both members of the equation,

$$\cos. \delta \sin. \psi = \cos. \varphi \sin. \eta$$

by $\cos. \psi$, we have :

$$\cos. \delta \text{ tang. } \psi = \frac{\cos. \varphi \sin. \eta}{\cos. \psi}$$

$$\text{whence } d\theta = \frac{1}{\cos. \varphi \sin. \eta} [\cos. \psi d\delta - dA - \cos. \eta d\varphi].$$

From this equation it appears :

(1) That the effect of an error in the observed altitude upon the time will be minimum when $\varphi = 0^\circ$ and $\eta = 90^\circ$, or when the sun is in the prime vertical.

(2) That the effect of an error in δ will be minimum when $\delta = 0^\circ$ and $\psi = 90^\circ$.

(3) That the effect of an error in φ will be minimum when $\varphi = 0^\circ$ and $\eta = 90^\circ$.

As an example we will assume that in a given observation of the sun or of a star the following small errors have been made, viz. :

$$d\varphi = +1'$$

$$dA = +1'$$

$$d\delta = -1'.$$

We compute the effect of these errors upon θ in three assumed cases :

	I.	II.	III.
$\varphi =$	$39^{\circ}54'$	$51^{\circ}30'$	$70^{\circ}0'$
$\delta = +$	$17^{\circ}29'$	$6^{\circ}37'$	$+ 44^{\circ}0'$
$A =$	$15^{\circ}54'$	$13^{\circ}40'$	$63^{\circ}30'$
$\theta =$	$83^{\circ}37'$	$58^{\circ}8'$	$10^{\circ}10'$
$n =$	$80^{\circ}16'$	$60^{\circ}15'$	$16^{\circ}32'$
$\psi =$	$52^{\circ}26'$	$32^{\circ}58'$	$7^{\circ}46'$
$-\frac{dA}{\cos. \varphi \sin. \eta} =$	$-1.32'$	$-1.85'$	$-10.27'$
$-\frac{d\varphi}{\cos. \varphi \tan. \eta} =$	$-0.22'$	$-0.92'$	$-9.85'$
$+\frac{d\delta}{\cos. \delta \tan. \psi} =$	$-0.81'$	$-1.55'$	$-10.19'$
$d\theta =$	$-2.35'$	$-4.32'$	$-30.31'$
	$= -9.4^s$	$= -17.3^s$	$= -121.2^s$

Ordinarily there will be some elimination between $d\varphi$, dA and $d\delta$, but the errors *may* all act in the same direction, as in the examples given. The third example may perhaps be considered an extreme case, but it will serve to show the necessity of a computed table of coefficients which shall serve as a guide in estimating the value of any given observation. An appropriate name for them would be *local coefficients of safety*.

(f) Errors arising from the error in the estimated run of the ship between the morning and the noon observations. The morning or afternoon observations give one co-ordinate of position, viz. the longitude; and the noon observations, the other, viz. the latitude. To get both co-ordinates for the same instant requires an allowance for the run of the ship during the intervening time. It is impossible to give any definite estimate of the magnitude of the errors thus introduced, but I suspect they will in general be found to exceed all the other errors combined.

In order to determine the degree of accuracy which may be expected *from the sextant considered as an instrument of observation*, when used by trained and skillful observers, I have collected such observations of this class as were available, both for time and for latitude. In most cases the average error has been found by comparison of the observations *inter se*. The observations made at Willet's Point have

an exceptional value, since they were compared with transit observations made at nearly the same time.

For Latitude.

Observer.	Place of Observation.	Average Error.	Range of Error.	Coeff.
Williams, 1793....	Cambridge	3.6"	14"	3.5
Paine, 1831....	Norfolk, Va.	9.6	40	4.2
	Richmond.	7.2	31	4.3
	Washington.	4.6	18	3.9
	Baltimore.	5.0	20	4.0
	Providence.	10.1	50	5.0
	Washington Territory.	7.2	46	6.4
	" "	10.9	41	3.8
1869....	Willet's Point	7.0	24	3.4
1871....	" "	7.8	29	3.7
1872....	" "	9.0	35	3.9
Hall and Tupman.	Malta	10.5	59	5.6
	"	10.2	37	3.6
	"	8.0	34	4.2
Hall and Tupman.	Syracuse	5.0	30	6.0
	"	9.2	36	3.9
	"	7.7	56	7.3
	"	7.1	28	3.9
	"	6.8	28	4.1
	"	6.7	30	4.5
Harkness.....	Des Moines	6.4	47	7.3
Means.....		7.6"	35"	4.6

For Time.

	s.	s.	
Hall and Tupman.			
Malta	1.6	7.3	4.6
Syracuse.	1.0	4.3	4.3
Newcomb	0.6	2.6	4.3
Harkness	1.1	4.7	4.3
Means.....		4.7	4.4

For the mean of any number of observations with the sextant, therefore, we must expect an average error of 7.6" in latitude and of 1.1s. in longitude, and the average liability to error of a single observation will be about 35" in latitude and 4.7s. in longitude. For sea observations an *additional* coefficient of 3 units will be none too great. No instrument of precision is so liable to constant errors as

the sextant. In all the cases given above these constant errors were thoroughly investigated and the resulting corrections were applied. It is not to be expected that this can be done with sextants used at sea. Even with observations on shore this instrument often acts in a most perverse and unaccountable way. The observations of Williams for the determination of the latitude of Harvard College in 1782-3 are so instructive on the point of agreement *inter se* and disagreement with the truth that I copy his separate results :

Latitude of Harvard College.

From the Sun.	From Fixed Stars.	From the Pole Star.
42° 23' 22.3"	42° 23' 25.1"	42° 23' 28.6"
25.3"	22.9"	27.7"
20.5"	34.1"	
	28.7"	
	36.5"	
	24.2"	
	28.6"	

The mean adopted was 42° 23' 28.5". By comparison with the latitude of the observatory this result is 64" too great, and yet the average deviation from the mean is only 3.6".

In 1825 Mr. R. T. Paine, perhaps the most skillful observer with the sextant in this country, if not in the world, found the value of the latitude of the Old State House, Boston, to be 42° 20' 30", a value which differed 118" from that given on page 297 of the Transactions of the American Academy, which was also the result of a sextant determination. In 1828-9 194 observations with sextant "Ramsden 1403" gave 20' 57.8", and 390 observations with "Ramsden 1375" gave 20' 57.9". Yet the mean of 442 observations between 1833 and 1873, divided equally between northern and southern stars, gave 21' 23", with a range between greatest and least for 37 dates amounting to only 7.5". This result is 7" different from the value derived by Borden in his Trigonometrical Survey of Massachusetts. It gives for the latitude of Harvard College 42° 22' 15", the true value being 22' 48.6". It is, therefore, 34" in error, while the earlier value is 59" in error. Mr. Paine's subsequent observations for the latitude of the Unitarian Church in old Cambridge gave 46.7", or within 2" of the truth.

It is well known that there is in general a well defined difference between results derived with the sextant from observations of the sun

and of fixed stars, though Mr. Paine always got substantially identical results. At Syracuse in 1870 Professor Harkness found the latitude from Polaris $7.1''$ to be less than from the sun. Professor Hall found a difference of $24.7''$. Struve, in his survey of Turkey, found that *Alpha Aquilae* gave latitudes $13''$ too large, and that this correction sometimes ran as high as $30.7''$.

It is obvious from this brief discussion that there is danger of placing too high an estimate upon the accuracy of the ordinary sextant considered simply as an instrument of observation. It is hardly necessary to say that agreement of repeated readings on a given part of the limb is not necessarily an indication of accuracy. The errors which are usually introduced in the graduation of a sextant belong to a class known as periodic errors. They are formed by successive increments of very minute errors, which it is perhaps impossible to measure individually, but which, by continual additions, may, at a given point on the limb, amount to a quantity several times greater than the error of observation.

The data at hand for assigning a limit to the actual errors of observation with the sextant at sea, taking into account all the errors to which such observations are liable, are exceedingly meagre. For the present I limit myself to the following discussions:

(a) Scattered through the volumes of the *Nautical Magazine* will be found records of the determinations of the longitude of various stations, chiefly in the West India Islands, made by various British naval expeditions. The data required are in many cases wanting, but sufficient are available to furnish a fair estimate of the average range of error. The chronometers employed were rated at the Greenwich Observatory at the beginning of the voyage, and the observations for time at the terminal stations were made in the usual way with the sextant. They were probably made on shore, though I can find no definite statement to this effect. Evidently more than usual care was taken with the observations and reductions. It is probable also that a sufficient number of observations were made at each station to eliminate accidental errors in a large measure.

Place of Observation.	Range between results given by different Chron'rs. <i>Miles.</i>	Range between results for longitude. <i>Miles.</i>	No. of Chron'rs.	No. of Days, Duration of Voyage.
Hong Kong Singapore, 1849-50....	—	10.0	5	18
“ “ “ 1852-53....	—	7.1	5	18
Funchal	2.0	1.5	10	11
Teneriffe	0.2	0.9	11	3
Port Prayo	3.2	1.3	11	10
St. Antonio	9.0	2.6	11	18
Cape Blanco	2.2	2.0	6	3
Cape Bojada	1.8	3.3	7	5
Goree Island....	—	2.6	—	—
Great Cayman	1.5	1.7	3	7
Cape St. Antonio	0.6	2.3	3	7
St. Salvador	9.5	13.8	6	6
Martinique	0.8	1.4	3	2
New Providence....	3.2	6.1	6	14
Guadaloupe	—	2.2	—	—
St. Martin's	—	4.3	—	—
Alta Vela	—	2.5	—	—
Trinidad	—	3.7	—	—
Barbadoes	5.3	4.9	5	13
Navassa Island	0.8	1.6	3	6
Island de Aves.	—	7.5	—	—
New Orleans	—	2.2	—	—
Chagres	1.6	2.0	2	15
Carthage	4.0	1.7	2	3
St. Martha	2.2	8.3	2	6
Curacoa	—	2.7	—	—
La Guayra	—	3.8	—	—
Archilla	—	2.4	—	—
Margaritta	—	7.1	—	—
Port Desire	7.5	1.2	7	19
Port Farrin	7.2	—	10	19
Callao	—	7.4	—	—
Valparaiso	6.0	1.4	13	17
Talcahuana	1.5	3.8	4	15
San Carlos	15.1	31.6	20	27
“ “	6.8	10.9	18	13
Panama	—	3.3	—	—
Guayquil	—	7.6	—	—
For 36 Stations.....	Mean	4.2	5.0	—
	Range	14.8	29.7	—
	Coefficient	3.5	5.9	—

(b) During the spring and summer of 1880 Officer W. H. Bacon, of the Cunard steamer "Scythia," kindly undertook for me a series of systematic observations from which the relative errors could be determined with considerable certainty. A complete series for a single day consisted of five sights at intervals of fifteen minutes, about 8 o'clock in the morning, five sights in the neighborhood of 11 o'clock, and five sights at the corresponding hours in the afternoon. Observations were also made when the ship was in known positions as often as possible.

This series of observations has an exceptional value on account of the conscientious fidelity with which the programme was adhered to and of the skill with which they were made. The relative errors were determined by comparing each position with the mean of the series, the rate being determined both from the morning and afternoon observations and from the log.

The results obtained are found in the following table:

LIMITS IN MILES.	Average Error from Observations at 9h and 3h.	Average Error from Log at 9h and 3h.	Average Error from Observations at 11h and 1h.	Average Error from Log at 11h and 1h.	Difference between Observation and Log at 9h and 3h.	Difference between Observation and Log at 11h and 1h.
	No Cases.	No Cases.	No. Cases.	No. Cases.	No. Cases.	No. Cases.
0.0.... 0.5.....	1	0	0	0	7	6
0.5.... 1.0.....	0	6	2	3	1	2
1.0.... 1.5.....	8	13	3	5	3	3
1.5.... 2.0.....	4	5	3	3	3	2
2.0.... 2.5.....	6	4	6	5	2	3
2.5.... 3.0.....	2	1	3	4	1	0
3.0.... 3.5.....	2	2	6	5	7	2
3.5.... 4.0.....	4	1	4	5	1	2
4.0.... 5.0.....	1	3	6	5	4	4
5.0.... 6.0.....	0	0	2	1	1	5
6.0.... 7.0.....	0	0	2	1	2	2
7.0.... 8.0.....	1	1	0	1	1	1
8.0.... 9.0.....	2	0	1	1	0	2
9.0.... 10.0....	0	1	0	0	1	2
10.0.... 11.0....	0	0	0	0	1	1
11.0.... 12.0....	0	0	0	0	2	1
12.0+	1	1	0	0	0	0

It will be seen that the results from these two investigations do not materially differ from those previously found. In fact, whatever the line of investigation pursued, we reach substantially the same conclusion, viz. that the average error of a single observation at sea is not far from three miles, and that the average coefficient by which this number must be multiplied in order to provide for every contingency of danger is 3.5.



NAVAL INSTITUTE. ANNAPOLIS, MD.

JANUARY 12, 1882.

THE COEFFICIENT OF SAFETY IN NAVIGATION.

BY COMMANDER P. F. HARRINGTON, U. S. N.

In the current volume of the Proceedings of the Naval Institute there appears an essay by Professor W. A. Rogers, of the Cambridge Observatory, on the Coefficient of Safety in Navigation, which is remarkable for the extent and thoroughness of its investigations, and valuable in the application of its results to the practice of navigation. The essay is based upon investigations to which the navy has not contributed, and it becomes important to consider the application of its conclusions to the ordinary course of navigation of men-of-war.

The coefficient of safety is defined as a number by which the average error of a ship's position should be multiplied in order to obtain the limit of possible error. If a ship be navigated in consideration of the limit so obtained, her safety is assured, so far as it depends upon an estimation of position. As determined by Professor Rogers, the coefficient is not strictly the ratio between the average error and the extreme error; but it may be so regarded in practice, upon the assumption that the least error in a series approaches nearly to zero.

In order to value the final effect upon the computed co-ordinates of a ship's position, the errors to which the *data* are liable may be classed and considered with regard to their influence in the computations and upon the estimated progress of a ship subsequent to the astronomical determination of her position. Those errors which affect the altitude as an element of the *data*, in observations at sea, are: first, instrumental errors, or those of the sextant; secondly, errors of observation; and, thirdly, errors of the tabulated dip and refraction.

The constant errors of the sextant are those of excentricity and graduation; and there are others, of a variable nature, which are due to the surfaces of the index and shade glasses not being parallel, to the setting of the telescope in a plane inclined to that of the instrument, to small inclinations of the index and horizon glasses, to an increase of the apparent diameter of the sun arising from the feeble power of the telescope, and to the effects of changes in the temperature. No error can arise from a prismatic form of the horizon glass. These errors differ in value for different sextants and at different times in the same sextant, and the total amount may or may not be inconsiderable. On shore, they may be examined and the corrections partially ascertained. Professor Rogers finds, from a series of observations on shore, a value of $7.6''$ for the average error in latitude and of $1.1s$ in longitude, and a value of the coefficient of four and seven tenths. In these observations the errors of the sextants were thoroughly investigated and the corrections were applied; but the results are open to the objection that they were obtained by a comparison with the mean of the series and not with a fixed value. The suggestion that the coefficient should be increased by three units for single observations at sea is not unreasonable; and from this we find the average liability to error at sea exceeding one minute of arc. It does not appear that the values reported are due entirely to the sextant and have reference to the altitudes of the bodies observed; but the character of the observations, of bodies on the meridian for latitude and near the prime vertical for longitude, justifies the assignment of the error to the sextant, since the error of the instrument entered directly into the altitude and was transmitted in its full amount to the computed latitude, or to the longitude with but little alteration, and since no other element could be erroneous. Other considerations, already stated, support the conclusion that the error of altitude at sea, due to the sextant alone, may exceed one minute.

The errors of observation include the inaccurate valuation of the index error, the personal error in the observation itself, the error of making the contact without the middle of the field of the telescope, and the special error of observation when the motion of the ship is considerable. The error arising from defective shade glasses may be avoided by using a shade cap over the eye-glass of the telescope. A screen of card board may be fitted over the eye-piece to shield the eye from the glare of rays, other than those passing through the

telescope. The total value of the errors of observation is neglected, necessarily, at sea, nor does it appear how these and the sextant errors may be considered except by the use of a coefficient of safety.

The most serious error is that of the tabulated dip. As the ship rises and falls with the swell of the sea, it is difficult to fix the height of the observer's eye; and, at moderate elevations, an error of four feet in the estimated height of the eye causes an error of about half a minute in the apparent altitude. Moreover, the dip is tabulated for a mean condition of the atmosphere. The path of a horizontal ray of light near the earth's surface varies greatly with the state of the atmosphere. From this cause the tabulated dip cannot be relied upon more closely than two minutes, when the eye is not elevated more than sixteen feet. The curvature of a ray of light in air at any point in its course depends upon the pressure, temperature, and rate of change of temperature; the third condition being in general the most important. When the temperatures of the water and air differ greatly, the variation of the apparent place of the sea horizon from its mean place becomes extraordinary. Variations of four minutes have been observed frequently, by the use of a dip sector or prismatic sextant, in comparison with a common sextant, and larger values have been found. The apparent horizon is depressed below or elevated above its mean place, according as the sea is warmer or colder than the air. For example, a large increase in the dip may be expected when traversing the Gulf stream in cold weather. The extreme case of displacement in a downward direction is illustrated by the *mirage*, where rays of lights pass from objects above the horizon, obliquely at first towards the earth, through successive layers of air of rapidly decreasing density and refracting power, and subsequently upwards, through air of increasing density, to the eye of the observer. The inverted image of the distant object is thus formed, while another image in its erect position is seen through the medium of rays passing more directly to the eye of the observer. In the polar regions images of distant objects sometimes appear lying in the air. This occurs when there is an extreme refraction in the contrary direction to that which causes the *mirage*, the layers of air near the sea decreasing rapidly in density with an increasing altitude. The tabulated refraction, which affects the altitude, also becomes more uncertain as the altitude of the body decreases. These errors of dip and refraction may be partially compensated by attention to the corrections for pressure and temperature of the air. The tables, however, are not perfect, nor

do they always agree. Two tables, now in general use, differ three minutes as to the amount of refraction near the horizon, for a mean height of the barometer and thermometer.

Some function of the algebraic sum of these errors affecting the altitude enters into every computation for position at sea, and the error in marking the time of an observation or of a comparison enters directly into every determination of longitude. What, then, is the probable average error of an apparent altitude taken under favorable circumstances at sea? Will it fall below two minutes? Or, rather, can the navigator feel assured that it is not greater? It may be said that these errors and others yet to be noted will offset each other generally to some extent; but by what test may this be known in practice, and what part of the uncertainty is removed?

With respect to the latitude, when it becomes an element of the *data*, it must be remembered that its accuracy depends, primarily, upon observations of the character already described, and, secondarily, upon the closeness with which the run of the ship is noted. The same is true of the longitude, whenever it enters as a part of the *data* in any observation off the meridian; and it will contain also the error in the assumed longitude of the place of rating, the error in the original determination of the chronometer correction, and the accumulated error of the rate; and some function of the algebraic sum of these will enter into the computation. If the longitude is sought, the computed value will contain directly the errors last enumerated, besides that resulting from the computation.

It has been shown by Professor Rogers that it is difficult to determine accurately, even with excellent instruments and under the most favorable circumstances, the geographical position of a point on the earth's surface. The values of the coefficient, obtained from series of observations on shore, will be received with surprise, but they must be accepted as true. What degree of dependence then shall be placed upon the result of a single set of observations at sea, affected by all the errors enumerated and in an unknown direction? Is it probable that the naval officer, using the best instruments and exercising the greatest care and selection in observations, computations, and methods of navigation, reduces the average and extreme errors of position to exceptionally small values? It may be asserted that the observations of naval officers are particularly accurate, but that is all that can be affirmed; and that the true position of a ship at sea is ever obtained from astronomical sources is exceedingly doubtful.

Formulas and tables, by which the effect of the errors referred to may be considered, will be found in any good work on Navigation, and there are many practical methods of reaching the same end ; but it may be interesting to notice, briefly, some rules by the observance of which the errors themselves may be reduced in amount and the general accuracy of the determinations may be improved.

Attention should be paid first to the situation of chronometers on board ship, with respect to the motion of the ship and to the proximity of any particular magnetic influences. Extreme motion of the ship disturbs the vibration of the chronometer balance and produces a change of rate. The balance may acquire a magnetic polarity ; and, in order to avoid an error arising from this cause, the chronometers should be placed with the twelve hours mark of the face in the fore-and-aft line of the ship, and they should not be removed for any purpose. The rates should be found for the constant position of the chronometers. When proper precautions have been observed to avoid these disturbing agents and also the danger of rude shocks, it is conceded that variations in the rate are caused almost entirely by changes of temperature. It is desirable, therefore, to guard against sudden and extreme changes, and, whenever practicable, to provide a place of uniform temperature. On board ship this is almost impossible, and the navigator must rely upon a close study of the chronometer and thermometer and of the law which connects their motions. During the progress of a cruise there will be found a gradual acceleration of the chronometer rate, which is independent of the effect of temperature. This is due to the thickening of the oil, which diminishes the amplitude of the vibrations of the balance. The amount of this change is peculiar to to each chronometer, and can be found only by experiment. Chronometers for the use of the navy are issued from the Naval Observatory at Washington, and are accompanied by a list of rates for different mean temperatures, found by actual trial and record. With this list before him the navigator will be able to anticipate in an approximate degree the changes of rates, by attending to the daily temperatures of the chronometer room and to the record of comparisons. Every chronometer has its own law of change ; and, further, the alteration of rate is not contemporaneous with but follows at different periods the changes of temperature. If this important duty of studying the character of each chronometer is neglected, a belt of possible error in longitude results from the average daily error and coefficient

of safety for chronometers. The average daily error is $\pm 0.72s$, and the value of the coefficient three units and two tenths. These quantities appear very large, but they are deduced from extensive and comprehensive examinations of chronometers; by a course of computation which offers no ground of objection.

The error and rate of a chronometer should be found from transit observations, or from equal altitudes of the same body when on opposite sides of the meridian. The whole error of longitude of the point of observation will enter into the chronometer error; but the rate may be closely determined by these methods, though the latitude and longitude are known only to an approximate degree. When such observations are impossible, the error may be found from single altitudes of the same body, on opposite sides of the meridian and near the same altitude, the mean of the results being adopted; and the rate from single observations of the same body, provided the conditions of atmosphere, altitude, and *azimuth* are not very different upon the two occasions. The error cannot be determined correctly from single observations of a body off the meridian. It seems unnecessary to argue that the sea horizon should not be employed in this work.

At sea, if the azimuth, or true bearing, of the body observed is less than forty-five or greater than one hundred and thirty-five degrees, the latitude should be computed from the observed altitude and the longitude by account. The error in the latitude, which results from the errors of the *data*, decreases as the body approaches the meridian in azimuth, and, when the body is on the meridian, it depends upon and is equal to the error in altitude. It should be remembered, however, that the *maximum* altitude of the sun differs from the meridian altitude, when the motion of the sun in declination is most rapid and when the ship's rate of movement in latitude is large, by an amount which, in certain latitudes, may exceed one minute. In the case of the moon, this difference may be much greater near the periods when the moon's declination becomes zero.

If the true bearing of the body is more than forty-five degrees and less than one hundred and thirty-five degrees from the meridian, the longitude should be the co-ordinate sought. When the body is on the prime vertical, a small error in the latitude is of no importance, and the error of altitude has its least effect, or, nearly, its amount converted into time. The error of the chronometer correction cannot be eliminated.

When the declination of the sun and the latitude are nearly equal and of the same name, the method of equal altitudes of the sun for longitude is highly recommended. The altitudes may be observed very near noon; and, in connection with the meridian altitude, the latitude and longitude are found almost simultaneously. If equal altitudes are lost, by reason of passing clouds, Littrow's method is still available, and is equally simple in computation.

It is recommended in Raper's Navigation that, when the altitude of the body is sufficient, it should be observed both from the opposite point of the horizon and from that under it, by the common sextant. Half the difference of the *readings* should be the apparent zenith distance of the body, eliminating the dip and the errors of the instrument. This assumes that the dip is the same at opposite points of the horizon. The difficulty of this observation would probably cause erroneous results, but it is suggested for trial, in deference to the high authority cited.

Observations at night are very unreliable, from the obscurity of the horizon and the difficulty of reading the sextant. But, during twilight, observations may be made which will reduce the uncertainty of the ship's position. The local time may then be found accurately from altitudes of stars east and west of the meridian and having similar zenith distances and azimuths, and a very close value of the latitude from altitudes of stars upon or near the meridian north and south of the zenith.

It is an obvious consequence of the preceding statements that the computation of position at sea to seconds of arc is unnecessary, so far as the accuracy of results is concerned, and that it may produce an appearance of precision and correctness not inherent in the methods. Aside from this tendency to engender an unreasoning confidence in the results, the habit of close figuring is a valuable one; and the use of seconds, in this class of computations, accords nearly with the usual practice of employing units of an order one lower than that required. Attention to minute details is almost essential to good navigation; and such attention leads at once to a just discrimination as to the degree of computation necessary.

When approaching a coast, off which the depths of water and character of the bottom are well marked, the lead becomes an invaluable aid to navigation. A good sounding and a line of position, taken together, are almost as useful as a lighthouse to an intelligent navigator. The following method of navigation by the lead is described

by Sir William Thomson. "Take a long slip of card, or stiff paper, and mark along one edge of it points at successive distances from one another, equal, according to the scale of the chart, to the actual distances estimated as having been run by the ship in the intervals between successive soundings. If the ship has run a straight course, the edge of the card must be straight, but if there has been any change of direction in the course, the card must be cut with a corresponding deviation. At each of the points thus marked on the edge, write on the card the depth and character of the bottom found by the lead. Then place the card on the chart, and slip it about until you find an agreement between the soundings marked on the chart and the series written on the card. The slight ups and downs of the bottom, even if they be no more than to produce differences of five or six fathoms in depths of, say, from five-and-thirty to fifty fathoms, interpreted with aid from the character of the bottom brought up, give, when this method is practiced with sufficient assiduity, an admirably satisfactory certainty as to the course over which the ship has passed."

The errors incident to the progress of a ship must now be considered. Imperfect steering, unknown or incorrectly estimated currents and leeway, and errors in the compass deviations, generally result in some wandering from the given course. The distance sailed is not accurately known, nor is it likely, in the absence of means of finding the set and drift of the current at all times, that the run of the ship can ever be implicitly relied upon. From the Dutchman's log to the patent log, the instruments have all failed to record exactly the progress of the ship. Napier's pressure log and Hogg's speed indicator show the rate of speed at any instant with accuracy; but for purposes of navigation we must look to improvements in those instruments which measure the total distance run by the ship, rather than to an indicator of the momentary speed. It is possible, however, that the pressure log, or the speed indicator, may be made to record continuously the rate of speed, by a mechanism similar to that of a self-registering tide-gauge, forming a speed curve which would present the rate at any instant and, by simple measurements, the distance run, to a high degree of accuracy. In further consideration of the coefficient of safety, it may be assumed that the average error in the course is one quarter of a point, and that of the distance one twentieth of the run by log. It is difficult to state the possible range of error from these causes; but every naval officer can recall occasions

when the place of the ship by account, after a run of a hundred miles, could not be depended upon within twenty miles in any direction, although good observations had been obtained at the beginning of the run. Yet this uncertainty corresponds to an error of one point in the course, or of one mile in a distance of five. Fortunately, in most instances, as in gales or known currents, the uncertainty becomes apparent and induces additional caution.

Of the large number of wrecks from preventable causes, and particularly of new iron or composite ships, the loss of a considerable *per centum* is caused by compass errors, such as unknown deviation due to change in the magnetic condition of the ship, to a change in magnetic latitude, to neglect of the heeling error, and to a change of course in high latitudes after the ship has sailed on one course for a long time. The causes of compass deviations and the character and degree of their changes are now well understood; and the losses from compass errors might be assigned justly to that class which includes ignorance or incompetence of the persons in charge of the vessels. The accomplished navigator foresees these subtle changes, understands their amount and effect, and provides the correction or remedy. The ignorant navigator very often makes a good guess, and sometimes makes a bad one and runs his ship by it into danger. But it cannot be doubted that a majority of vessels of the class here referred to, and many others, would have avoided disaster by the use of a coefficient of safety.

The erroneous position of the coast-line upon a chart may be considered, with regard to an approaching vessel, as represented by an equal error in the position of the vessel and in the opposite direction. For example, if a coast is five miles in longitude eastward of its place as represented on the chart, it is relatively the same as if the ship's actual place is five miles in longitude westward of its computed position by chronometer. In effect, the error in a coast-line may be dealt with as an error in the ship's position, and it thus becomes connected with the value of the coefficient of safety. The determination of longitudes by electric telegraph, by Lieut. Commanders F. M. Green and C. H. Davis, under the direction of the Bureau of Navigation, will be of incalculable value to the commerce of the world, in multiplying points of reference for chronometer errors and in the correction of charts.

Considering all the various sources of error, the average error of the ship's position at sea, as found from a single astronomical observa-

tion, is required, and also the average hourly increase in this quantity due to causes operating subsequent to the astronomical determination. The application of a coefficient will then show upon a chart the circle of possible error; and, in approaching or passing a danger, the course of the ship should be laid from that part of the circle nearest to the danger, and pursued more particularly as the character of the soundings, currents, and absence of lights, render uncertain the proximity of the danger. The extreme errors are most difficult to ascertain at sea, from the want of a true standard to which to refer the computed positions, at the times these errors are likely to occur. It seems, therefore, the best course to adopt a value of the coefficient, say of four units, based upon the discussions of Professor Rogers, and to approximate to the amount of the average error. Every navigator may find a value of this error; first, in the manner indicated at the close of the essay, and, more accurately, by a comparison of computed positions with those found simultaneously with the observations, by cross bearings, or by horizontal angles by sextant between three points when passing well established landmarks. These comparisons should be obtained under all possible conditions, of atmosphere, of height of the eye, of altitude and azimuth of the body, and of motion of the ship. When a lighthouse or other good landmark is first sighted in the course of a voyage, its correct magnetic bearing and distance should be carefully determined and compared with the computed or chart bearing and distance, and in this manner the error in position at the end of a run may be found. For example, if a known point of land is seen at a true distance of twenty miles upon a magnetic bearing which differs one point from the chart bearing (corrected for deviation but not for variation), the error of position is not less than three and nine-tenths miles. If the bearings coincide, the computed distance may be in error, and the amount of the error may be ascertained frequently by attention to the progress of the ship and the change in the bearing of the object. A table of average errors could then be formed, in which the classification should regard mean altitudes, latitudes, height of the observer's eye, and variations of the atmosphere; and such results would be very useful, if the work be conducted systematically and reported in detail. Observations when at anchor in a roadstead, where there is a large arc of sea horizon visible, ought not to be included in such an investigation, as the nearness of the land will modify the condition of the atmosphere, and as the circumstances are different in other respects from

those desired. Even in a roadstead, a series of observations would produce interesting results, and would probably lead most people to join Professor Rogers, in the respect he feels for the judgment of that sea-captain, who placed the average error of a ship's position at five miles.

The use of the coefficient is not very dissimilar to the best characteristic of Sumner's method, in connection with a single line of position. The prudent navigator assumes that his ship is not at the point on the chart indicated by the computed latitude and longitude, nor even upon the line of position, but at any point within a belt formed by drawing parallels on each side of the line of position at such a distance as to include all the probable errors of altitude, chronometer, etc., incident to the particular circumstances of his observation and progress.

The essay of Professor Rogers will be read with profound interest by naval officers. It indicates a field of investigation in which the efforts of naval officers may be usefully exerted. Its practical conclusions and warning ought to be impressed upon every man who is permitted to lay a vessel's course. The term Coefficient of Safety in Navigation should be received into the vocabulary of nautical phrases, to be associated in the mind of the navigator with the errors which he must estimate and correct, and to become a momentary element in that watchfulness which is the price, though not the assurance, of safety upon the sea.

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